

1778a
M E M O I R S

OF THE

Royal Society ;

Being a New ABRIDGMENT of the

Philosophical Transactions:

Giving an ACCOUNT of the Undertakings, Studies, and Labours of the LEARNED and INGENIOUS in many considerable Parts of the WORLD; from the first Institution of that ILLUSTRIOUS SOCIETY in the Year 1665, to the Year of our LORD 1735 inclusive.

The whole carefully abridg'd from the Originals, and the Order of Time regularly observ'd, with a Translation of the LATIN TRACTS, and the Theoretical Parts apply'd to Practical Uses; also an Explanation of the Terms of ART as they occur in the Course of the Work. Being a Work of general Use to the Publick and worthy the Perusal of all MATHEMATICIANS, ARTIFICERS, TRADESMEN, &c. for their Improvement, in various Branches of Business.

By Mr. B A D D A M.

V O L. VI.

Illustrated with a great Variety of COPPER PLATES.

L O N D O N:

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M.DCC.XL.

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Fig. I.



Fig. II.



Fig. N.

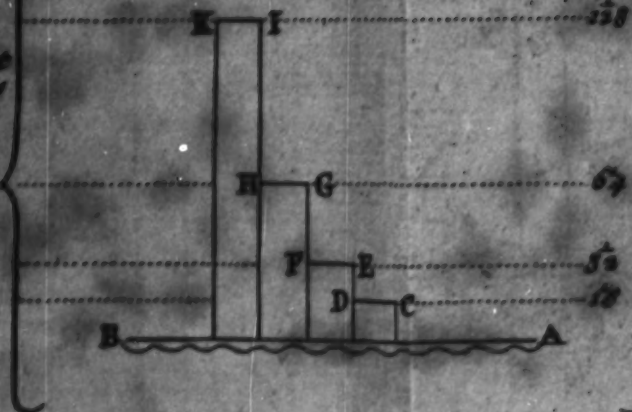


Fig. V.



Parts of an Inch. Fig. VI.

The
different
Distances
of the
Planes
from
each
other.



The
Heights
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G. Smith del.

Fig. III.



Fig. VIII.

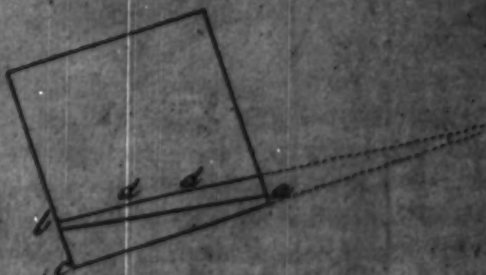


Fig. VI.



Fig. IX.

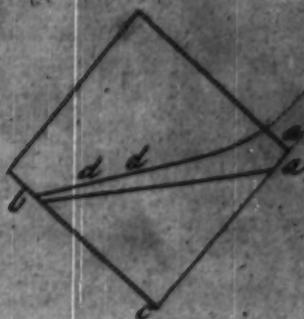


Fig. XI.

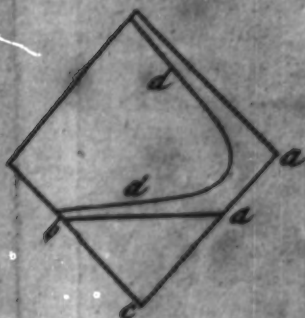
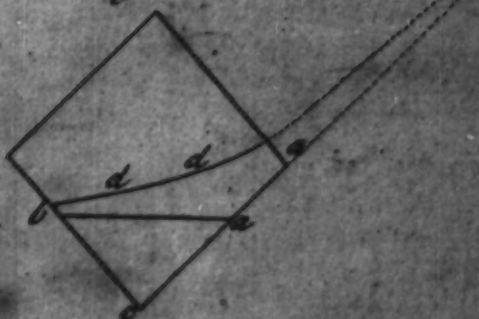


Fig. X.



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MEMOIRS OF THE ROYAL SOCIETY;

Being a New ABRIDGMENT of the
PHILOSOPHICAL TRANSACTIONS.

Observations in Natural History in Scotland; by Mr. Lhwyd.
Phil. Trans. N^o 337. P. 97.



We travell'd in the *Highlands* of *Scotland*, thro' *Cantire*, *Argyle*, and *Lorn*, besides the *Illes* of *Mac-ich-ormic*, *Mul*, and *y Columb kil*; and in the *Lowlands* thro' *Glasgow*, *Sterling*, and *Edin-burgh*; in the *Highlands* we found the people every where civil enough; We met with several *Inscriptions*, but none of them *Roman*, nor, indeed, ancient; however, we copied all we met with of 200 years standing, &c. for the sake of the orthography of the *Irish* names, which are writ differently from what they are now; we also took figures of several broaches, or silver and brass *fibulae*, or clasps used by the women to clasp their *Kaleriv*, a garment, answering our night-rails; they have great variety of amulets, many of which, if not all, were certainly us'd by the *Druids*, and so have been handed down from parents to children ever since; some of these may be rendred

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A 2

into

into English, 1. Snake-button. 2. Cock-knee-stone. 3. Toad-stone. 4. Snail-stone. 5. Mole-stone. 6. Shower-stone, and 7. Elf-arrow.

1. The Snake-button is the same with that describ'd in the notes on *Denbighshire* in *Camden*, by the name of *adder-beads*; but there is a great variety of these, as to colour and ornament; inso-much, that both in *Wales*, and the *HIGHLANDS* Mr. *Lbwyd* observ'd at least 50 different species of them; he could hear nothing of them in *Ireland*, tho' they are tenacious enough of all their old customs; so that he concludes, that either the *Irish* had no *Druids*, or that their want of snakes frustrated their advancing that imposture amongst the people; not only the vulgar, but even gentlemen of good education throughout all *Scotland* are fully persuaded that the snakes make them, tho' they are plainly glass. 2. The Cock-knee-stone is an *Echintes pileatus minor*, of flint; which they firmly believe to be sometimes found in the knees of old cocks. 3. The Toad-stone is some pebble, remarkable for its shape, and sometimes for its variety of colours; this is supposed to prevent the burning of a house and the sinking of a boat; and if a commander in the field has one of them about him, he will be sure either to win the day, or all his men fairly die on the spot. 4. The Snail-stone is a small hollow cylinder of blue glass, composed of four or five annulets; so that it resembles a midling *entrouche*, as to shape and size; this amongst others of its mysterious virtues, cures sore eyes. 5. The Mole-stones are rings of blue glass, annulated as the aforesaid snail-stones. 6. They have the *Ombrie pellucide* (which are chrystal balls, or hemispheres, or depressed ovals) in great esteem for curing of cattle; and some on *May* day, put them into a tub of water, and besprinkle all their cattle with that water, to prevent being elf-shot, bewitch'd &c. and 7. As to this elf-striking, their opinion is, that the fairies (not having much power themselves to hurt animal bodies) do sometimes carry men away in the air, and furnishing them with bows and arrows, employ them to shoot men, cattle, &c. the arrow-heads they ascribe to elves or fairies, are exactly the same chipped flints the natives of *New England* head their arrows with at this day; and there are also several stone hatchets found in this kingdom, not unlike those of the *Americans*; Mr. *Lbwyd* never heard of these arrow-heads nor hatchets in *Wales*, nor in *England*; these elf-arrow-heads have not been used as amulets, above 30 or 40 years, but the use of the rest is immemorial; whence he gathers, that they were not invented for charms, but were once used in shooting there, as they are still

still in *America*; the most curious, as well as the vulgar throughout this country are persuaded they often drop out of the air, being shot by fairies, and relate several instances of it.

Near *Glasgow* we found two fossils *totogenere* new; one resembling the small joints of a lobster's arm, but much longer; the other somewhat like large *glossopetrae*, or perhaps, like the *Mucro* of a *Pinna marina*; these figured stones are found there in an iron-stone, tho' Mr. *Lbwyl* never saw them in that kind of matter in *Wales*; we found both shells and *Entrochi* turned to that substance, having changed their matter, and much of their shape: Near the same town, searching for these fossils, he found in the middle of the lime-stone some *Cochlite*, composed of flint; but the *Conchite* of spar were so much changed from the shape of shells, as hardly to be known, were it not from others in the same place, that retained their shape more entirely; the principal of the college of *Glasgow* shewed us stones he had procur'd for the library; with *Roman* inscriptions; we copied these and several others elsewhere of the same date, of which there are some printed in *Camden*, tho' not very correctly, and others not yet published: One Mr. *Sutherland* gave Mr. *Lbwyl* specimens of the *Chamepericlymennum*, *adiantum acrosticon*, and *pyrola alpine flore Europea*; Mr. *Lbwyl* found abundance of the *vitis Idea foliis myrtinus crispis Meretti* for several miles together in that ——— and that part of *Mul*, next to y *Columb Kil*; it is very different from the common *vitis Idea sempervirens fructu rubro*, being a larger plant, and much more branch'd, the leaves of a crisp'd surface, and the berries (which he was told it retains all the year over) like those of holly; going up one of the high hills of *Mul*, we found *Rhodia radix*, *pes Gari*, *coryledon hirsut*, *vaccinia rubra*, *sedum Alpin. trifido folio*, and *Achimilla Alpina quinquifolia*, which he had never seen grow spontaneous; we found in this island, a curious *Fucus arboreus*, with a ruffled stalk.

An Account of the Plague in Dantzick in 1709; by Dr. Gottwald. Phil. Trans. N° 337. p. 101.

THIS dangerous and destructive distemper had raged for some years in several cities, towns and villages of the kingdom of *Poland*, where it swept away vast numbers, and even left some places quite desolate; it first began near *Pinczow* or *Pickzow* in 1702, soon after the unfortunate battle between the *Saxons* and *Swedes*; the next year it appeared here and there towards

towards *Cracow* and *Russia*, and had already caus'd a great mortality near the *Hungarian* mountains, call'd *Crapack*, till it went eastwards to the upper *Volhynia*, and again westwards to *Lemberg*: In 1704 it raged very violently in these two Palatinates; so that *Lemberg*, the capital city of *Russia*, lost a vast Number of its inhabitants, and many to save their lives were oblig'd to fly from it; in autumn it spread in the neighbouring places, that lie to the west and south, beginning at a village call'd *Radymino* on this side *Jarislav*, and afterwards invading other places, seated betwixt the rivers *San* and *Volodavara*, it spread till it came towards the *Samber*: In 1705 it left *Lemberg* and went west and north to *Great Poland*, thro' *Jarislav*, *Sieniatze*, *Zamose*, and other adjacent places; and continued in that part of the Country, as far as *Posen*, all that year: In 1707 it enter'd the city of *Warsaw*, where it destroyed that summer a vast many people: In 1708, it came nearer towards *Polish Prussia*, and the latter end of *August* it broke out in *Thorn*, where it continued till the beginning of next year, and swept away a considerable number of men; this approach made those in *Dantzick* very apprehensive of danger; publick prayers were order'd in the Churches, and the magistrates left nothing undone, that could tend to the common safety; commerce and communication with the infected, and even suspected places were forbid; no sort of merchandise, or effects that came from such places were allow'd entrance, especially such sort of goods, as might easily receive, and retain the contagion; as wool, raw leather, furs, beds, &c. All strangers and Travellers were strictly examined, and none permitted to enter without sufficient proofs, that they came from healthy and uninfected places; all the inhabitants were cautioned neither to hold correspondence with, nor on any pretence whatsoever, either to harbour those of infected places, or to go to them; these and other necessary precautions were taken, and by publick edict enjoined from *July* 11. 1708, to *February* 27. 1709. Notwithstanding all which, the distemper gradually insinuated itself; for, in *March* 1708, there died out of one district of the old town, call'd *Raambaum*, seven persons, the eighth remaining was a young girl, who by order of the magistrates, was sent to the hospital, having already some bad symptoms, as bubo's, about her; which being yet unknown, one would not presently have taken it for pestilential, but rather venereal; but they soon shew'd their epidemic kind, by seizing on other children that were in the same room: In the said month the Dr. had under cure a *Polish* lady, come hither from *Jarislav* labouring

labouring under a cachexy and tympany; in a fortnight's time she recovered so well, that soon after she went to church; but it was not long before she fell ill again, being seiz'd with a slow fever, and a sudden decay of strength, and died within eight days; when they came to wash her dead body, there was found on the lower belly a brownish red swelling, about the bigness of a small hen egg, which the Dr. afterwards judged to be a *Furunculus*; so much he could then conjecture, that it was not a common swelling; for, it was surrounded with a lead colour'd circle, from which proceeded several blue rays in the adjacent parts: However, this was not a sufficient ground to demonstrate, or to give public notice, that the contagion was already amongst us; but in the mean time care was taken to prevent, as much as possible, the farther progress of this distemper; the streets, waters, and houses were order'd to be cleans'd from all manner of filth, and nastiness: On *Sunday* the 16th, the Governours of the hospital desiring Dr. *Gottwald* to be then physician in ordinary, or at least to give his assistance, by reason of the great numbers of patients therein, which increas'd daily; he went next day to the hospital, and enquiring for the surgeon in ordinary, he was inform'd that he died the day before, but they did not know of what distemper; in viewing the patients, he found ten in a room together, of various ages, some of which had bubo's, others carbuncles, others gangrenous ulcers, which one cannot always judge to be pestilential; in another room there were upwards of 20 children, from six to 12 years of age, all which, excepting four or five, had either pestilential bubo's in the groin, armpits, and about the neck, or else carbuncles on the arms, thighs, legs and other parts of the body: After the Dr. had perfectly informed himself of the state of the hospital, he took his leave, having first recommended to the surgeon such necessary medicines as he judged proper in these cases to be given inwardly; but the Dr. was of opinion, that these were symptoms, if not of a plague already insinuated, yet at least of something, but little inferior to it, and certain fore-runners of that destructive distemper; upon this he call'd to mind, what he had observ'd in his travels at *Venice*, in those that came from the *Levant*, and had performed their quarantine, who also had the plague; and tho' the surgeon did not at this time think that it was any thing so dangerous, yet soon after the distemper seiz'd himself, and all his family, of which himself his wife, and all his children died: From this time the malady, and number of patients daily increas'd, and mostly in the outer parts, and suburbs of the town.

June

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June the 26th, the Dr's daughter, about six years and a half old, began to complain of an unusual head-ach; he presently gave her some bezoardic drops, with volatile cephalic spirits; in four hours after she was much altered, her eyes stared, her extreme parts were distended, and violent convulsions ensued, tho' they did not continue long; but afterwards she lay, as if she were paralytic, and could not be made sensible; what medicines soever were made use of, they proved ineffectual, so that she died the third day following; the same day also the Dr's wife fell dangerously ill; but in a few days got over the worst, and tho' she was confined to her bed a long time, yet she at length recovered: The Dr. found, that all the efforts he could make were too weak to resist the violent progress of this distemper; for, one might daily perceive how the poison got strength, and consequently, that the disease was harder to cure: Thus the distemper grew predominant, and by the end of August had spread almost over all the city; all possible care was taken to supply the poor (which are very numerous in this place) with necessaries both of food, and physic; to have the streets, and dwelling houses kept clean, and neat; the communication of the sick with the sound prevented as much as possible; and the dead buried timely, and in the place appointed for them, by the authority and direction of certain persons the magistrates had appointed for that purpose, called *Provisors of health*: Besides, the pest-houses were opened, and well provided with all manner of necessaries, as also with overseers and servants, for defraying the charges of which considerable sums of money were collected; several persons of condition hired particular houses for their servants, and others made up convenient apartments in their gardens, and procured servants, and nurses to attend them, that in case of infection they might be taken care of; the pest-waggons, and chairs were employed from betimes in the morning till late at night; the former to carry away the dead to be buried, and the latter to convey the sick to the hospitals; besides the ordinary church-yards, there were others made without the walls of the city: In short, every thing was ordered very conveniently, and so well and carefully looked after, that without it the infection would have destroyed a much greater number of people; and tho' this is a very populous city, yet during the contagion, they did not lose near so many, as was reported, viz. 40,000 persons; the falsity of which account plainly appears from the bills of mortality, together with the number of christenings, and marriages this year, as they were carefully taken, and published by order of the magistrates.

In

In *August* and *September* the plague raged with greatest violence, when several eminent men, to the great detriment of the public, lost their lives; but of the chief magistrates they lost but two, and as many of their judges; there died about $\frac{1}{2}$ part of their divines; of the physicians, and apothecaries none at all; of the surgeons in ordinary only two; but of their assistants, especially such as belonged to pest-houses, a vast number; the principal citizens suffered very little; but the garrison a great deal, tho' the officers escaped pretty well; but the handycraft, and common tradesmen, as well masters as journeymen, apprentices, porters, and labourers were very much diminished, and in 1709 there died to the number of 24,533, in which are likewise included all such as were buried without the town, and some of another jurisdiction; of which no true, and exact account could be had; but as much as could be collected, was published by Dr. *Shelwig*.

There were two things remarkable; the first was, what Dr. *Shelwig* observed, viz. that the plague decreased in the same proportion, as it had increased; for, in *June* the number of the dead was 319; in *July* it rose to 1313; in *August* to 6139; and in *September* to 8303, which was the highest degree of mortality; after this the numbers again decreased; so that in *October* they were 4932; in *November* 1961; and in *December* 584, and so on gradually decreasing; the other thing remarkable was, that but few of the people of quality, and condition died of the contagion, in comparison with those of the common, and ordinary sort; which may be attributed (next to divine providence) to the different number of rich and poor, and the great care and precaution the former took to avoid it: Yet it is to be observed, that during the whole time of the contagion, the citizens enjoyed beyond others of their neighbours, a free, and uninterrupted church service; their gates were never shut, but free passage in and out allowed to all; they had plenty of provisions, and all manner of necessaries for the subsistence of man and beast, by a communication with all neighbouring places, and countries; nor was their commerce, and traffic entirely interrupted, but there was a moderate trade all the while; tho' the posts did not go, yet their communication was not quite cut off; and tho' their exchange, and markets were less frequented, yet their churches were in proportion the fuller.

Having thus observed the regulation used in the course of this distemper, the Dr. subjoins something more of its progress, tho' it did not always observe regular successive periods: He observed

above; that in *March* 1709 the first unusual mortality happened at the *Raumbaum* in the old town; from whence it spread immediately into the hospital, where he saw the first symptoms of the plague; here it lay lurking for some time, so that they could not perceive it make any further progress; but towards *July* it began to shew itself more openly, passing from the hospital, situated north-east, towards the west, and at first it seized on the suburbs, which lie beyond the fortifications, and run out in length upwards of $\frac{1}{4}$ of a *German* mile, and called *Schidlitz*, besides other parts that lie transversely, and have different names; from the suburbs it went east, and west towards the town, and seized those people chiefly who lived near the fore and hinder *New-Garden*; it continued this course, only inclining a little more towards the south, and partly infecting the places that lie between the fortifications towards the south, and partly those that are in the same line without the walls, as *Schottland*, *Obre*, *St. Albrecht*, besides some part of the *City* within the walls, lying towards the *Plaga*, and likewise called the suburbs; it also spread very much south, east, and north, to distant places, till it came near the open sea, and ravaged the lands, and places belonging to the town, as *Hochte*, *Werden*, and *Nabzung*: As the distemper had in this manner surrounded the whole town, and infected every quarter of it; so their neighbours on the frontiers had likewise received the infection; but it was observable, that in no district of the town was the number that died less than in that part properly called the city: Tho' in the course of the infection it spread itself, and run in a circuit, as it were, yet its motion was not so transient in shifting from place to place, as not to continue where it had first laid hold, only with this difference, that it did not so severely infest the places it had at first possessed, as those it had entered later; and this was very well perceived, when it was raging the fiercest in the pest-house, *Hinterbidblitz*, and other places.

The Dr. accurately observed every particular that happened, in order to find out, if possible, the natural source of this malady, and by what medicinal means it might be resisted: Any one who peruses the authors, that have writ on this subject, cannot want good information; but as to this particular point, there is such a disagreement among them, that one shall never be able to learn from them the true natural cause of the plague, because it often proceeds from different causes, and oftentimes the same common cause is attended with such different circumstances, as to render it dubious,

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That the plague is a poison, or rather carries a poison along with it, is acknowledged by all physicians; but of what kind, and nature it is, and whence it proceeds, very few can agree. It is well known, that it has a twofold operation; in such a manner, that the blood of the infected is sometimes thereby coagulated, and sometimes dissolved, according as the humours of human bodies are disposed; and yet they are both alike pernicious; if it coagulate, the juices stagnate, and the progressive motion ceases; if it dissolve, then the natural connexion, and cohesion of the particles become colliquative, and incoherent, and the spirits gain a free exit, and leave the body motionless.

That the air was infected, during the contagion at *Dantzick*, is certain; not that he means a general infection, as if the air was tainted, or corrupted by a supernatural power in such a manner, as to taint, and infect all things breathing; for, by this means no soul could have escaped; but as it is a subtle, moveable, and every where expanded body, it attracts, and receives all effluvia and exhalations, as a sponge does water, and likewise imparts them, by means of its motion, to other bodies; so that, as a communicative medium, by its entrance into our bodies, we receive whatever it carries along with it; the Dr. therefore explains, how the air comes to be of this, or that disposition, or constitution; by what means its pure and natural temperament is altered; and how far, and with what particles it is impregnated; for according to that it must be more, or less impure, and so affect accordingly.

It is generally observed, that the plague commonly ensues after great battles; the reasons alledged for it are, that the effluvia proceeding from the vast number of dead bodies infect the air, whereby mankind are afterwards infected; how much more then must they be infected, when there is an actual contagion among them, notwithstanding that it owed its first rise to such corruptions, as by means of the air are brought from foreign parts, an instance of which we see in goods, and merchandizes; he says, the infection of the air must be still greater in proportion to the actual increase of the pestilence, when so many thousands of people die, and several continue to lie rotting for many days above ground, and others buried without a coffin, and but a very little way under ground; besides, the heat of the weather (for these casualties generally happen in summer) causes the bodies to corrupt sooner; and there is also a great difference between those that die in battle, and those that die of the plague, the former being sound bodies, but the latter infectious and a mere

mass of corruption, before they die, and must consequently infect the air more than the former.

What Dr. Gottwald has said concerning an infected air, is confirmed by the testimony of all who have writ on the plague by experience; and they have likewise observed the circumstances, and symptoms to be the same as they appeared to him: *Horatius Lumbisanius*, in his treatise of the plague, asserts, that there are only two kinds of it, and that both of them proceed from the air, for which he refers to the authority, and consent of the ancient physicians; *Matthæus Ramelovius* in his *Speculum pestis perpolitum*, p. 9. also mentions impure, poisonous air, which easily receives venom from one man, and imparts it again to another; not to mention others: But *Paul Sorbait* shews this by singular instances in his discourse of the plague at Vienna 1679, where he says, 'that a vast number of people, and I myself among the rest, have sometimes felt in the groins and under the arm-pits violent pains, but without any tumour, and these intermixed with palpitations of the heart; all which went off again in time.' These the Dr. thinks were certain signs of an infected air, only that it does not make so great an impression on some, as on others; and in the same discourse, Quest. 13. he again repeats his opinion in these words, 'that since the air is infected, it may easily affect the above-mentioned parts of the body, especially the heart, the plague being often called *Morbus inqualis*;' with him likewise agrees *Andreas a Keib* in his *Loimographia*, containing several very remarkable things: What *Sorbait* asserts, Dr. Gottwald found to be true by experience; otherways he knows not to what cause to impute his having felt, during this contagion, the very same pains which he did several times one after another, insomuch, that sometimes he was not able to stir; besides, when he had come from places, where people lay yet unburied, or from infected houses, he frequently found a palpitation at his heart, a pain in his head, and anxiety, with a reaching to vomit, but without bringing any thing up: In the beginning of October he fell ill of a violent catarrh, which obliged him to keep his bed; on the third day it turned to a salivation, which continued for three hours so violent, that his gums, and mouth swelled, as if he had taken mercury: but next day he recovered, and was well again, having never been affected in like manner either before, or after; whence he concludes, that he received

received it from the poison in the air, only it had not power enough over him, otherways it might have suffocated him.

April 27, 1703, Dr. Gottwald was bit by a viper in the fore-finger of his left hand, of which in a moment after he grew out of order, and at length fell into convulsions; having immediately after the bite sucked his finger, his face swelled, and his mouth almost closed up; yet notwithstanding, in three hours after, the convulsions began to cease, and he gradually recovered.

But to return, that the air was actually infected, appears from this, that several of the inhabitants, tho' they took never so much care to prevent being infected, kept continually at home, used all manner of preservatives both inward and outward, suffered none of the infected to approach either them, or their servants, yet nevertheless they caught the infection; besides others that conversed with people, that were healthy, and well, and without the town, in an open free air, and well situated places, (but perhaps, by chance looked out to see some body or other carried by to be buried) took the infection, fell ill, and died; so that the Dr. cannot in the least doubt, but that the air is infected; and that by means of some morbid effluvia, wherewith it is impregnated, it also infects, and destroys mankind; concerning which see the learned Dr. *Laurentius Bickstadtus's Disput. Physiolog. de peste*: There were several other indications of the disorder of the air; such as several unusual *Phænomena* that were seen in it, the state of the animals that mostly live therein, &c.

Authors generally make a distinction between the signs of a future, and a present plague; concerning the former, the famous historian *Casper Hedio* in his *Chronica* has related several odd appearances that happened in the stars, the air, the earth, and the waters in *Germany, Italy, France, Poland, &c.* as certain signs of an approaching plague, which soon after ensued: Tho' Dr. Gottwald could not disprove the consequences, drawn from these, and such like relations, since the event confirms the truth of them; yet according to him they are but uncertain signs, and as such are esteemed by *Pet. Sybillinus*, and others of the learned: However, the Dr. gives an account of some of the unusual signs, that happened before the plague came; of this kind was the extraordinary number of spiders, that were found in such abundance the preceeding year, viz. 1708 towards the east, that a country gentle-

gentleman assured the Dr. when one day he happened to be looking after his workmen, he was covered all over with them; the same thing is likewise observed, as a fore-runner of the plague by the learned Dr. *A. Q. Rivinus* in his *Tr. de peste*; Dr. *Gottwald* also observed the same sort of spiders near his house; the winter following, which was that before the plague, they had such a frost, as never happened within the memory of man; it began on the second day of *Christmas*, and continued till *Easter*, during which time there fell such vast quantities of snow, that they were obliged twice to employ carts to carry it out of the streets; notwithstanding, the Dr. cannot think these sufficient indications of an approaching plague; because this distemper very often follows upon a mild, as well as a hard frost; several examples of which may be produced from *M. Reinhold Carike's* account of the plagues, that happened at *Dantzick* in 1352 and 1427, &c. But the signs that are observed in the air, at the time when the plague rages, are more evident; and especially those observed in mankind themselves, will easily evince the infection of the air: On *August* the 11th at 12 o'clock the Dr. first observed a stinking mist, like a thick cloud, but of short duration; but at 4 o'clock it returned from the north-west so very thick, that it quite darkened the air, and obstructed the sight; it was neither blue, nor grey, as other common mists, but of a blackish yellow, like the effluvia that rise from the effervescence of oil of vitriol with oil of tartar; after it had reached the middle of the town towards the south east, it inclined westwards, and there emitted a very noisome stench: The second sign of a corruption in the air, as Dr. *Sorbais* calls it, appeared about the beginning of *October*, viz. a blue fiery globe, which came from the north west, about 11 o'clock at night, and shot towards the town as swift as an arrow out of a bow, and seemed to fall over against the town towards the south; in its flight it shewed a great light all over the town; the third sign of an infected air was the accurate observation of the learned Dr. *Schekwig* in his treatise before-mentioned, viz. that in *July* the crows, daws, sparrows, and other birds, which at other times are to be seen in the town and about the gardens in vast numbers, were all fled, and none of them to be seen till *November*; the same was likewise observed of the storks, and swallows; and Dr. *Gottwald* himself saw none of these birds all those four months.

These

These things happening at this time made the Dr. often inquisitive about the reasons of them; yet he should hardly have been persuaded, that they had any dependance upon the plague, since they may, and sometimes do happen without it, if there had not appeared other more dangerous, and certain signs of a plague in mankind itself; these, indeed, were very many; some external, others internal; the former never without the latter; and the latter but seldom observed without the former, only that sometimes the external signs do not appear, till the patient be dead; both have often been taken for diagnostics, as also for prognostics, experience confirming, that upon such and such signs, and symptoms nothing but death could ensue: But great regard ought to be had, first to the signs themselves, and then to the symptoms; for they may be spurious, and without malignancy, tho' by the diagnostics you might take them for pestilential; and especially the time must be carefully observed; for several signs, that in the increase, and at the height of the distemper appeared dangerous, in the beginning, and decrease of it were mild, and without any danger; for the distemper began, and increased gradually, and decreased in like manner; but the middle was the worst, and most violent; for at first the *Bubo's* were more common than the *Furuncles*, *Carbuncles*, and *Vibices*; afterwards the *Petechie* were more common than these; tho' during the whole time of the contagion, they were never entirely separate, so as to appear sometimes the one without the other; at last the *Petechie* and *Carbuncles* went off, but the *Bubo's* continued last of all.

The *Bubo's*, which are to be reckoned the first of the external signs, lie very deep in the skin, and are at the beginning hard, immoveable, and round; afterwards they grow longer, and may be moved; they do not look red outwardly, till they are drawn, and brought to maturity; they are generally found in the groins, arm-pits, and about the neck; most of them come with a very violent, cutting, and pricking pain, accompanied with heavy symptoms (which the Dr. calls internal signs) such as pain in the head and back, shivering colds interchanged with heat, anxieties, faintness, and frequently also with bilious vomitings; according as there is more or less malignity, the symptoms are proportionably more, or less violent; sometimes they are very mild, and the *Bubo* proceeds without any great pain: *Furuncles* differ from the common *Bubo's* in this, viz. that they appear mostly in the fleshy parts, to the
number

number of 5, 7, or 9 on one body; they are sometimes red, and swell to a greater height than the *Buboes*; their pain is very violent, and therefore disturbs the patient's rest; the other symptoms are much the same, as in *Buboes*; and tho' they are not always alike, yet they are never without a fever, and are attended with pain in the back, and belly: *Carbuncles* and *Anthracas* are much more pernicious, and of various kinds; the Dr. only mentions four sorts of them, the first shows itself somewhat eminent, and rising, of a dark brown, the uppermost skin appearing somewhat dry, as if it were burnt, and withal it has a lead-coloured circle; in the beginning it is frequently no bigger than a pea; but if not prevented it increases in a short time to the bigness of a crown-piece; it is inwardly moister than the following species, and may be more easily separated; its seat is generally in the fleshy parts, on the shoulders, sides, hips, neck, and also on the arms and legs: But the second sort lies somewhat deeper, and seems a little more depressed; the eschar is in the middle of it, which is entirely dark and ash-coloured; full of small cuts, as if were burst, by too great a dryness, with a strong lead-coloured circle, behind which the sound flesh looks red, and shining; it eats the flesh about it very much, and fixes its roots very deep; in its separation it feels drier than the former, and may be taken out by pieces; it is generally fixed where the flesh is thickest, as on the buttocks, the calf of the legs, and under the short ribs towards the back; these two species burn violently strong beyond all the rest, insomuch, that a red hot iron can hardly cause more pain; and indeed, the patients (not to mention other symptoms that accompany them) are almost killed by the mere pain of them. The third sort is not very large, nor of a large circumference; at first it appears like a blood swelling, not so black as the former, the skin being also somewhat wrinkled; in its increase small blisters arise on its middle and form an eschar in small clusters, which as an ingenious physician observed, were small *Carbuncles*; they are commonly situated in membranous, and tendinous parts, as towards the knees, behind the ears, upon the toes, &c. The fourth species is the most curious, but very deceitful, as *Purman* in his treatise of the plague has well observed; *Job. Bapt. Sironius in Miscel. Med. curios. Tr. I.* calls them *papula livida, ulceroſa, & pallida*; it appears with a high blister, which is yellowish, as if it contained corruption; there is first round it a red, and then an ash, or lead-coloured circle; the blister

blister soon falls, and within you see the *Carbuncle* hardly as big as a pepper corn, which continually eats deeper, and wider; these are seated upon the cartilaginous parts; Dr. Gottwald found them near the pit of the stomach upon the *Cartilago ensiformis*, and short ribs: All these four species of *Carbuncles* take deep root, and burn very violently in the beginning, but the two former more than the latter; the symptoms attending them are violent, tho' not always sensible, but so much the more dangerous; as generally, restlessness, *Deliriums*, sudden loss of strength, pain in the head and back, anxieties, inward burning heat, thirst, &c. The first onset is frequently attended with a shivering, bilious vomiting, &c. according to the greater, or less power of the latent *Miasma*; the symptoms are always more or less violent.

The *Petechiæ*, or malignant spots, which are at all times, as is well known, very dangerous, did especially in this contagion rage violently; he also observed four distinct species of them; the first look like flea-bites, and therefore, have been called by some authors *Pulicæres*; they break forth reddish, and soon changing their colour, grow brown, and at last black; they are round, and spread all over the body, excepting the face, where they are not always found: The second species appears in the shape of lentils, and therefore, they are called *Lenticulares*, and are likewise at first ruddy, but in about 24 hours they change colour, and grow dark, and ash-coloured; they spread, as the former, all over the body: The third sort appears in large round spots, of the same colour with the former, only they are found but here and there upon the body; sometimes they are also intermixed with the lentil kind: The fourth species is not unlike the measles, and spreads all over the body; after two or three days you find them shoot into little blisters, rising to a head, but containing no matter; they dry away the fifth day, at which time the patient's death is not far off; after they are dried away, the skin is rough, and much like that of a smok'd goose, only not quite of the same colour: To these might be added a fifth species, mentioned by Dr. A. Q. Rivinus in *Tra. de Pest. p. 51*. These do not appear till after the patient's death, either in points, or spots; but seeing they differ in nothing from the afore-mentioned, excepting that they first appear after death, they can scarcely be reckoned another species; they shew themselves mostly on the back, and breast, and give plain indications of a malignity, that killed the patient: Many dismal,

and pernicious symptoms do also appear at the breaking out of these several *Petechiæ*; as pain in the head, and loins, vomiting, *Diarrhœa's*, palpitations of the heart, great anxieties, faintings, shivering in all parts of the body, which are frequently succeeded by heat, or sweat, *Delirium's*, epileptic fits, lethargy, a dismal hippocratic face, staring eyes, bleeding at the nose, immoderate menstrual fluxes: In short, the symptoms are so many, and various, that it is impossible to observe them all: The next are the plague stripes, or rays, called by *Job. Bapt. Sitonius*, *Vibices*, and by others, *Molopes*; they are not observed, before the latter end of the patient; for death itself attends them; and this was wont to happen unexpectedly, tho' the symptoms were tolerable, and at the same time hopes of recovery; yet they shot upwards like lightning, from the breast to the face, all in spots of various colours, as blue, green, brown, and yellow; first covering the face as high as the nose, and from thence spreading farther to the fore-head, disfiguring the patient in a frightful manner; his eyes grew stiff, his tongue trembled, his speech gradually ceased, and inwardly there was great anxiety, and confusion; from all which the struggle between life and death might well be observed: Dr. *Gottwald* observed the *Fire-bladders* only in two patients, and that in the beginning of the contagion, both which recovered; and therefore he does not think them so dangerous, as M. *Purman* describes them to be, unless he mistook their species and property; they appeared to the Dr. as broad as a shilling, of an irregular height and figure, with a clear wrinkled skin, as if shrivelled by fire; at length they emitted a small quantity of moisture, and vanished in a few days; he observed them only on the belly, thighs, and legs; they came forth with a small cold and succeeding heat, and with pains in the head, and back, and weariness; and these were the external signs.

As to the symptoms, seeing they appeared very various, tho' the *Exanthemata* were one and the same, it is impossible to describe them accurately; yet by reason of the prognostics, he divided them into several classes for his own private practice, that he might the better judge of the event: In the first class he placed all those that were not in themselves dangerous; in the second those that were doubtful, and had various events, both good and bad; in the third those that were wholly dangerous: The symptoms in themselves not dangerous, were pains in the head, a little shivering or cold, a tolerable

terable heat, nauseating of victuals, thirst, the belly distended with *Flatus's*, anxieties, dejectedness, pains, and stitches behind the ears, in the temples, on the shoulders, and heaviness in all the limbs: The dubious symptoms were palpitations of the heart, shortness of breath, anxieties, faintness, looseness, vomiting, dryness of the throat, restlessness, a continual fever, *Delirium*, &c. The symptoms altogether dangerous, were sleepiness, and lethargy, palse, epileptic fits, cramp, bleeding at the nose, irregular menstrual fluxes, miscarriages in child-bearing women, sudden loss of strength, a stiffness, and shivering thro' all the limbs, burning hear, staring, and watery eyes, continual inquietude, with great anxieties, external coldness of the limbs, and inward heat, with dryness of the tongue and throat: But it is to be observed, that tho' no one of these symptoms singly by itself can absolutely determine either good, or bad; yet the Dr. could truly affirm, that if any one of those of the last class; as for instance the lethargy, or an unusual shivering thro' all the limbs, happened to any patient at the first onset, he seldom failed in his prognostication; therefore, the Dr. adds some further observations, that may serve instead of aphorisms in practice, and which have been useful to him in prognosticating the event of this distemper; but first he mentions a remarkable thing, that was very common at the time when the plague raged, and is not to be reckoned among those signs, that happen by accident, *viz.* several people, not excepting the stoutest, were frequently struck with a fear, horror, and anxiety, insomuch, that they perceived a violent trembling, and beating of the heart, and a pain in the back; many of them would immediately fall into despair, and thought the plague had already seized them; this was more frequently observed, especially in *July, August and September*; it seemed strange to the Dr. at first, but he found that it also had its natural causes, which were excited by some secret passions of the mind, and therefore, might also be remedied by natural means; chearful, and encouraging discourse to rouse and comfort the spirits, contributed a great deal to the cure; but there were a great many upon whom this had no effect, who remained inconsolable, and melancholy so far, that at last they died: At length this fear prevailed so much, that even the physicians themselves left the town; the nearest relations would not venture so much as to visit those that were thus possessed, or give them any assistance.

The particular accidents, as far as the Dr. could observe in his own practice, and from thence prognosticate, are as follows; the *Bubo's* were in the beginning of the contagion much more dangerous than afterwards; and those that happened on the left side were more pernicious than those on the right; which very well agrees with what *Rivinus* p. 54. has observed: *Bubo's* did not always require to be drawn, and extracted, nor would they sometimes be drawn, or forced outwards, but were divided by good emollient medicines: It was better not to lay plaisters presently upon the *bubo's*, but to stay till the fifth day, and then it might be done with safety: Carbuncles seated on the nervous parts, prov'd more dangerous, than on the fleshy parts: Where carbuncles did not quickly come to separation the case was dangerous: Carbuncles without the patient's being particularly sensible of them, and without heat and great lassitude, were followed by death the fifth day: Carbuncles generally after the fifth, or ninth day did well permit of a cure, but requir'd great care, especially that the patient should not catch cold: If a *bubo* happened near a carbuncle, it was a good sign, and less dangerous than if the carbuncle was alone: Carbuncles hard by *Petechie*, or spots, were generally mortal: The *Petechie*, or spots resembling flea-bites, were mildest, and best of all; nay, some patients went abroad with them, but seldom with any benefit to themselves: *Petechie* that did not break forth before the fifth day prognosticated death: Such *Petechie* as are called *Ienticulares* and *Purpuratæ* were at this time all mortal: Occult *Petechie* caus'd certain death: *Vibices*, or plague stripes were infallible signs of death: As soon as a shivering, with pain in the head, and back, bilious vomiting, and great lassitude happened, then was the patient seiz'd with the plague: If the shivering was violently strong over the whole body, and a trembling in all the limbs, and the pulse withal weak, then death infallibly ensued on the third day: *Hæmorrhages*, or bleeding at the nose, or irregular menstrual fluxes, whether they happened the first, second or fifth day, or even the seventh or ninth were always dangerous, and mortal: Generally all evacuations, if they came with sudden loss of strength, and an unequal trembling pulse, were not critical, but colliquative, and occasion'd death: Ordinary menstrual fluxes, if there were no *Bubo* or *Furuncle*, were not dangerous: Generally all beginnings of this distemper, which affected the senses, or nervous parts, were dangerous, and signified death: Great inclination to sleep, or lethargy at the first onset, was a dangerous sign; and tho' the patient bore up under it the

the second, or third day, yet he rarely escap'd death: All relapses were dangerous; if they happened soon after the fifth day, the seventh was the last; if after the ninth, then the patient fell into an hectic fever, and died some weeks after: If the patient on the third day, after the onset of the distemper, appear'd strong, and talk'd much, yet had an inward timourousness, then he died the third day after; if the patient was taken with a shivering, not succeeded by heat, but great weakness, and without any visible sweat, had a dry throat, a dry breast, and a tickling cough, the case was very dangerous: It is to be observ'd, that in this distemper, no critical signs appeared; yet several accidents had their certain determinate days.

Before the Dr. concludes this part, he takes notice of the signs of the urine, which several learned men give little credit to; nor does the Dr. himself take them to be infallible, as to the diagnostics, and prognostics of the distemper; but when compar'd with the rest, they give great light to the physician, so that he may be able to form a more certain judgment of the state, and event of the disease; for instance, a clear and pale urine, which we otherways call raw, and unconcocted, if it does not soon change, and give a sediment, is dangerous; a bloody and putrid urine, let it look otherways as it will, if it had on the surface a pale red circle round the glass, was at this time generally a sign of malignancy; nay, tho' the urine was not stirred, and only this circle seen, it never prognosticated any good, but was at least a sign of great weakness; lixivious urine, without sediment, and half concocted, and also in small quantity shew'd a want of strength, and a fatal event.

As to the indications for practice, great care was taken to enquire after, and procure whatever might tend to the preservation and cure of the people: A good diet was recommended to them; that the superfluous, and luxurious abuse of victuals might not add fuel to the plague; common brandy (the *Panacea* of the vulgar) was advis'd against, and forbidden; and in its stead the moderate use of *French* brandy was allowed them, and not without great benefit; for thereby the spirits were cherish'd, and fear, and anxiety banish'd, all violent passions of the mind proving destructive to health; pot-herbs, garden-fruits, and the like, which are generally flatulent, as also very salt, and high seasoned meats, were not allowed.

The medicines in pharmacy, which were made use of, were many; besides those published by order of the magistrates, and written by the physicians in ordinary, for the preservation of the people,

people, in which they recommended their specifick prescriptions: Of simple internal medicines, those in greatest esteem, were *radix Angelicae, calamus aromaticus, Myrrha rubra, sulphur, nitrum, ruta, &c.* Of compounds, the chief were *Machandel-chalk, Theriaca, Mithridate*, various preparations of pestilential vinegars, morsels, troches, all manner of prepar'd powders, also gun powder, &c. Some made use of the *Medicina stercorearia*, and those very carefully drank their own urine: Amongst other preservatives, the following sent from *Thorn*, was in great esteem, of which the prescription is as follows. *R. Aloes Hepat. 3vi. Terra sigillat. 3ls. Theriac. Venet. 3vi. Rhabarb. opt. 3ii. Croci orient 3ls. Zedoar. 3ii. Myrrh. rubr. 3iii. Agaric. 3i Gentian. Dielamn Rad. Tormentil. Camphor. Castor. Rad. Angelic. aa. 3ss. misce:* Stamp these species small, and pour upon them a pint and a half of good *French Brandy*; then let it in a warm place, in a well clos'd bottle, and extract the tincture, of which take 12 drops as a preservative; but those that are already sic'd with the plague, or any malignant fever, must take a spoonful of it, and sweat upon it: As to this *Arcanum*, *Dr. Gottwald* can neither recommend the composition, nor the operation of it, not having found, that it performed any thing extraordinary in the distemper, nor was it so beneficial as much more simple medicines: Such as made use of their own physicians, and followed their prescriptions, fared, no doubt, much the better for it; as having their bodies seasonably prepar'd with alexipharmics, bezoardics, antiscorbutics, diaphoretics, diuretics, gentle laxatives, &c.

Dr. Gottwald's preservative was an essence, prepar'd of the species of the essence of woods, saffron, aloes, and red myrrh, extracted with spirits of wine, adding thereto the *Mixtura simplex*; which he found beneficial, not only towards prevention, but likewise in the cure, as shall be shewn hereafter: *Dr. Godfrey Siuve*, an ingenious physician of *Dantzick* made use of this medicine with a great deal of benefit: *Dr. Achatius Muller*, at the very time when the plague was at the height, had great success with his *Acetum Antimonii ex Minerâ*, which doubtless, was mix'd with bezoardics; besides his *Essentia preservativa*, *Dr. Gottwald* gave sometimes a bezoardic powder at night, consisting of the *bezoardicum compositum Wagneri*, or else this bezoar mix'd with *Specificum cephalicum Michaelis*; and if there were the least appearance of the distemper, he ordered his *Sulphur minerale confortans*, mix'd with a bezoardic or digestive.

Among

Among the various compositions of bezoardic powders, the following was none of the least considerable; of which the Dr. order'd very frequently to be taken as much as would lie upon the point of a knife; especially if his patients had a terror upon them, or by reason of melancholy found any alteration about them: To others he ordered it as a preservative, to be taken in a morning mix'd with beer; the prescription is as follows. *R. Rad. Tormentill. Scorzonera. Petasid. Symphit. Aristol. rot. Lign. Guaiac. Colubrin. aa. ʒiii. Rad. Curtum Zedoar aa. ʒils. Myrrh. rubr. ʒi. Corall. alb. Conch. mar. aa. ʒss. Succin. flav. Terr. sigillat. rubr. ʒ alb. Carn. Viper. Oculor. cancr. Bot. Armen. Corn. Cerv. ust. aa. ʒss. Antim. diaphor ʒvi. Nitr. Amodin. min. ʒiii. M. Dosis. ʒi ad ʒi.*

Those who were oblig'd to be frequently, or constantly with their patients, made use of a vinegar, prepar'd for that purpose (of which there are several compositions) which they took inwardly, and likewise smell'd to: Others made use of *Rosula sublinguales*, of which also there are several preparations; Dr. Gottwald prefer'd to them the *Placentule senectutis*, which Dr. Sylvius Bocco has described in his *Obs. curios.* p. 402. And when he visited his patients, he generally held these in his mouth: to take away the bad stench, which was very frequent in the patient's chambers, the rooms were smoak'd with juniper, vinegar, powders, candles, and especially with gun-powder; but this last must be carefully made use of: The Dr. generally had red myrrh, and saltpetre, boil'd in vinegar, and order'd it to be pour'd upon a hot tile, to smoak the room with; and this was done with considerable benefit.

Evacuations were likewise reckon'd by some amongst the preservatives; but as that common proverb, viz. *vis vi est repellenda* is not always allowable, but often noxious and dangerous in the practice of physick; so likewise these violent means occasioned in the time of the plague; a great deal of harm: A gentle laxative taken once in a fortnight, or three or four weeks, according to the temperament of the body, could not be amiss; but it must be done with great care, and the patient keep within doors; Dr. Gottwald for that purpose, generally made use of the *Essentia laxativa*, first invented by his father; it consists of the *Extract Pulpæ Colocyntidis cum spiritu Vini parat. Part. 2.* and the *Solutio Resinæ jalappæ ʒ Scammonii ʒa pariter cum spiritu Vini rectific. fact. Part. 1.* These must stand to digest in *Balneo Vaporis* for some days: Other laxatives were likewise suc-

successfully made use of, if but ordered at a proper time, and the patient took care of himself.

But much more regard ought to be had, as to vomiting and bleeding; if we have regard to the indications, we cannot resolve upon either of these without danger; bleeding is, indeed, generally allowed, if the patient be plethoric, and there be blood enough; which, by a different, and extraneous matter, may be easily brought to an ebullition, and so rarified, and consequently, require a larger space: and then there is an indication, that it wants vent: But during the contagion, very seldom was any such commotion perceiv'd in the blood, but rather a concentration of the spirits, and a want of the natural heat; to prevent which, the preservation rather than the diminution of the mass of blood was to be considered; besides, these can be no indications how to preserve health, considering what the Dr. observed, as a mortal sign in this distemper, *viz.* hemorrhages; if the hemorrhage had happened critically, and not symptomatically, which presently indicated a colligation, and commonly threatned death, then thence might be formed a judgment, that bleeding might contribute somewhat towards preservation: The Dr. was obliged to take up with such signs as nature offer'd, who, of herself, sufficiently shews what is most beneficial, and also what is most noxious; because succeeding experience confirmed, that the indications, according to the external signs, were contrary to each other, and therefore could not instruct, either how to preserve, or cure.

Sometimes bleeding in the plague, has been reckon'd a thing indifferent; but then the indications were very obscure, or entirely fallacious; yet the Dr. thinks it were better omitted than ordered: Sometimes one cannot know, whether the patient has a disposition to receive the venom or not; if he has, bleeding is absolutely dangerous; if not, it signifies nothing; nor is it any consequence, that all those that receive the pestilential venom must be overpower'd, and kill'd thereby: The poison, doubtless, retains its strength to operate; but it finds something that resists its operation, according to the temperament and nature of the body; which resistance physicians place in the natural heat, and a free operation of the spirits, which cannot well be preserv'd by bleeding, tho' they may not always be disturb'd thereby: Notwithstanding, the question is not, whether bleeding be generally pernicious, but whether it can be allowed to be a preservative in time of a contagion; because, as the Dr. has said above, it is abso-

absolutely dangerous, and pernicious to such as are infected, or dispos'd to infection.

As for vomiting, it is not only of less use than bleeding, but much more dangerous; especially when the contagion is at the height; for, 1. It is observ'd, that the contagious venom is not to be search'd for in the stomach; because it finds other ways, thro' which it may insinuate itself into the body; and tho' some persons should receive some of this venom with their aliment into the stomach, yet by reason of its subtilty, it does not stay long there, but presently seizes, and attacks some more noble part; for, it is well known, that the effect of the venom is presently to be observ'd in the blood; which it disturbs, or alters in the *Crisis*, or motion, and consequently disorders all the functions of the whole body; whence it is, that the onset may not be so easily observ'd, tho' its progress may: But if vomiting be only designed to cleanse the first passages, then you must know, that the distemper seldom or never has its rise in the inferior parts of the *Abdomen*, but commonly affects first the nervous parts, by which means the inferior parts are likewise affected, and causes several bad symptoms, and convulsive motions; whereby it is true, the first passages are sufficiently cleans'd, but it is to the great prejudice and ruin of the patient; whence it may be concluded, that evacuations without great necessity are absolutely useless.

2. We must avoid such preservatives, as produce indications, not for the better, but for the worse; the commotions caus'd by vomiting are not natural, but brought about with force and violence; for which reason *Galen* observ'd, even in his time, *vomitória habentia locum, ubi morbus vestibus est expellendus*; whereby in a great measure the more noble parts may be weaken'd; nay even the spirits (in which consists the vital essence) are dissipated, and diminish'd, which still are the greatest preservatives of the body; and if these are disturb'd, as they certainly are by vomiting, then the body is render'd the more dispos'd for receiving the contagious venom.

3. It is the opinion of the most learned, and experienc'd practitioners, that vomits in their nature, and power differ little from ordinary poison; how is it then possible, that one poison should qualify another?

4. And lastly, if they be considered as auxiliary remedies, then let them be administer'd with the greatest caution possible, especially at a time, when they choak and kill so many symptomatically; if an argument *a posteriori* may be allowed conclusive, then what was said before of bleeding, may also be said of vomits, that they prognosticate an unhappy event to the disease,

and that they have not the least use imaginable in preserving from contagion; sometimes it happens, that a man overcharges and distends himself with victuals and drink, and that crudities lie in the stomach; in which case there are plain indications for the use of vomiting, to prevent crudities fouling the blood; and at such a time it is not dangerous.

Some may say, that intemperate persons may happen to be as well at the time of a plague, as at other times; this is undeniable, but it is certain, that such crudities in the body can neither increase, nor promote the contagious *Miasma*, seeing, as has been observ'd above, it immediately seizes the nerves, and consequently occasions convulsions, and spasms, which frequently scour, and cleanse the first passages more than is necessary, insomuch, that when nothing more can be expell'd out of the stomach, the gall itself is sometimes brought up with such violence, that the very *Viscera* are ready to be torn to pieces: Whence it appears, that no remedy can be more dangerous, than vomits; therefore, great care ought to be had in giving vomits, and seeing them well administer'd, otherways the patient is soon sent out of the world.

Having spoke of evacuations, which are otherways call'd universal medicines, the Dr. resolves the following query; viz. whether, the *Ptyalismus*, or spitting can, and ought to be consider'd, as a preservative in the plague? For, *Jacobus Johannes Wencestaus Dotzenski* has extoll'd it mightily in a particular treatise, and recommends it as the greatest preservative: Dr. *Gottwald* was himself of a phlegmatic constitution, and very inclinable to such an excretion; and upon smelling, or tasting any thing nauseous, he was oblig'd to void spittle in large quantities, which very often happened to him during this plague, when he had been in places far from being agreeable; yet he could not say, that this spitting (which in him proceeded naturally, in others must be excited by medicines) was anyways serviceable to prevent his being infected; by reason that the pestilential venom does infect the spirits, and mass of blood more than the *Lympha*, from which the *Saliva* is secreted; besides, persons infected did not swallow the venom, or convey it to the stomach by the throat, but rather receiv'd it thro' the *Aspera Arteria*, and lungs: In the mean time he could not say, that spitting was so injurious; but he supposes it unnecessary to force it by peculiar medicines; for, there is a great difference between a venereal, and a pestilential venom; the latter presently attacks the spirits; and if it does not quickly overcome them,

them, it may likewise enter the *Lympha*, and then a salivation may be a remedy towards curing the distemper; but the other *viz.* the venereal venom is not reckoned at all dangerous in this age, nor does it want any preservative, provided one avoid the occasion of it.

The Dr. proceeds in the next place to the cure, and relates the remedies he found answer in this distemper: It is certain, that at the time of the contagion, a physician cannot always exactly observe every indication; several prognostics appear'd very often at the first onset, which so infallibly came to pass, that there was no need to attend to the diagnostics; and tho' necessary means were not for that reason neglected, yet the distemper baffl'd all the care and remedies used: If the patient was to be treated, as is usual in other distempers, then it would have been necessary to have considered what was most proper in the three-fold fountain of diet, pharmacy, and surgery; but the case was such, that the patients would frequently abstain from those things which might be noxious to them, of their own accord, by a mere natural dislike; notwithstanding some would venture too soon (when in a good way of recovery) into the air, which presently occasion'd a relapse, that cost them their lives: Several in the beginning abstained from eating, and drinking; and at length they desired to have a glass of wine or the like; commonly small, and well brew'd beer agreed best with the infected; as to sleeping, and waking, seeing they came symptomatically, the patients could not by all the persuasions either hinder, or procure them; as for much motion, the Dr. never advis'd his patients to it, but rather to keep in bed, and keep quiet, unless when out of fear, they would unseasonably give themselves up to much rest, and then it was best to make them stir about a little, to prevent melancholy: As soon as ever he was satisfied, that his patients had the plague, he absolutely avoided purging, vomiting, and bleeding; for, generally those to whom they were administred, tho' they seem'd ever so robust, did not escape death.

In his practice, Dr. Gottwald took the following method in curing his patients. 1. To extinguish the pestilential venom. 2. To mitigate, and assuage the violent symptoms. 3. To preserve the natural strength, as much as possible; for the first of these he us'd bezoardics and sudorifics, such as *Tinctura bezoardica Michaelis*, *mixtura simplex*, *spiritus digestivus Schroder*, *Spiris. Bezoard. Bossii*, *Essentia iheriacalis cum & sine Camphir*, *Essentia lignor. Elesthar.*, *Diascord.*, *Fracast.*, *Pulvis bezoard. Wagneri.*, *Pulv. Bezoard.*

Bezoard. Senn. Bezoard. Mineral. Anticell. Poterj. Antim. Diaph. cephal. Michaelis, &c. He ordered the above mentioned *Essentia preparativa* to be given in the beginning of the distemper, six times in 24 hours, from 25 to 75 drops, according to the strength, and age of the patient; and every twelfth hour the patient took one of the bezoardic powders, which did a great deal of benefit; the Dr. likewise found the *Bezoardicum Senn.* and *Bezoard. Wagneri* very beneficial; or else this, or the former mix'd with the *specific. cephalic. Musb.* or *Bezoard. mineral. Anticell. Pot.* with the addition of *succin. alb.* and *Myrrha rubra. flor. Sulph. Lac Sulph. Nitram Antimon. Sulphur Mineral.* and according to circumstances he added sometimes *Crocus orient.* The *Electuarium Diascord.* *Fracasor.* dissolv'd in *Aqua distill. Millefol. Sambuci, Tormentill. Scorzoneræ. Scordii, Cardui bened. Angelicæ Limpnellæ, Ratae, Galegæ, &c.* with the addition of the *Essentia citri liquida*, and made pleasant with a syrup of that kind, makes a good sudorific, and alexipharmic potion: The following potion he prescrib'd with great benefit to his patients *R. Aq. Scorzon. Scordii, Tormentillæ, Millefol. aa ʒi. Aceti Bezoard. ʒss. Essent. Lignor. ʒii Mixtur. simpl. ʒi. Elixir proprietat. ʒss Syr. de symphis. Fernel. ʒii f. mixtura* for three doses: Upon the taking of this mixture, the patient was made to sweat, and not to drink any thing cold; and after the sweat he took a cordial powder, prepar'd in much the same manner with the *Species cordiales* & *solares alexipharmaca Zwelferi* or the Dr.'s *Sulphur minerale*, mix'd with *Magister. Margar. Lap. Hyacinth. Smaragd. Bezoard. orient.* and other cordials; Whenever his patients were very much inclined to be sleepy, and to doze much, he added to the former medicines volatile salts; but if on the contrary he perceived any colliquations, then he us'd to add *Crocus orient. Theriaca caelestis*, and other pectorics, but with caution; if convulsions and spasms happened, then cinnabarine medicines were successfully applied; in hemorrhages the *Martialis* and the *Tinctura antimonii* were very beneficial.

Dr. Gottwald in the next place gives an account of such medicines as were made use of externally, and were capable of mitigating the extraordinary symptoms: As when the patient is first taken, his limbs are quite stiff, and benumb'd with cold, as it were, by the violent attack of a cold ague-like fit; in which case were presently applied hot bags, fill'd with oats, or the patient was rubb'd well with warm cloths, to bring him to warmth again; anodynes proved ineffectual against inquietude and Delirium; but *Fol. Ruta, salicis, plantaginis, scordii, serpillis, &c.* mix'd with salt, and moistened with rose vinegar, clapp'd warm about

About the head were of service to a great many: *Vertigo's* and Pains in the head were often mitigated with the ordinary *Species pro cucupha Dissp. Berol.* of which one part, mixed with a quarter part of *Nitrum antimoniale* and *Fol. Ratae, Portulacae* and *Endivia*, of each half a part, is to be tied about the head: Against dozing and sleepiness, a blister was applied on the neck, and snuff blown up the nose, especially some quickening salts, or spirits, rubbed in the nostrils: Violent *Diarrhoea's* were indications of outward convulsions, and were frequently stopped with a topical medicine of *Theriaca, Mithridate, or Oroscian*, mixed with vinegar, and spread upon leather, or linen, and applied warm to the navel: Bleeding at the nose was one of the most dangerous symptoms, and could not by any mere external application be stopped; and if it were, it signified nothing, especially, if it happened the third day; yet the Dr. found it sometimes very useful to put lint moistened with vinegar of roses into the nostrils, and tie a small bolster with the *lapis hematitis* moistened with vinegar upon the hollow part just above the nose between the eye-brows; or to put rue and red roses, stamped, and sprinkled with raspberry vinegar between a piece of linen doubled, and to apply it to the neck: When the throat was very dry, barley-water was used, and sometimes *Sal prunel.* added and sweetened with syrup of mulberries, granats, &c. and sometimes used without any syrup: In such as suddenly lost their strength, and had their spirits dejected, all manner of cephalics, and cardiacs were used, and whatever was thought would strengthen the patient; and if he desired a glass of *Rhenish*, he was never denied it; but this appetite seldom happened till the patient was past recovery; otherways cordial juleps were given him, mixed with bezoardics, if the circumstances of the distemper required it; or the *Confectio de Hyacintho* and *Alkermes*, mixed with *Aqua cerasor. nigr. flor. Boraginis, Lillior. convall. Rubi idaei* &c. to which were added sometimes the *Elix. Citri, Granator. Acetosiratis citri, cydoniorum, mororum*, &c. also ptisans and milk-whey; as also emulsions made of almonds, and *semen paeoniae, cardus benedicti, aquilegiae*, &c. Hartshorn jellies made with barley water, French, or *Rhenish* wine, with the addition of *syrup. acetosiratis citri, diamori, ribium*, &c. all which do very much refresh the patient: Among the powders the following are likewise of use; *lapides pretiosi, coralla rubra, unicornu verum, lapis bezoard. orient. Margarit. orient. Magisterium Margarit. orient. Magisterium corallo-*

corallorum, &c. and what is better than all the rest, the *sulphur minerale* if well prepared, as it has been by the famous *Basilius Valentius* and *Franciscus de la Boe Sylvius*, and communicated to the Dr's father as a particular *Arcaenum*: Besides these, *Epithemata* were applied to the head, heart, and pulse, which were of some efficacy; for by the pleasantness of their smell they very much refreshed, and recreated the spirits, and so retrieved the patient: Both heating and cooling applications were of use, and were applied as the physicians thought proper, and the symptoms would permit; as the *Aqua carbunculi, cephalica Carol. V. Apoplectica, odorifera Reginae Hungariae, Lillior. convall. &c.* mixed with the *Confectio Alchermes, species cordial. Essent. Citri sicca, Essent. Ambrae sicca*, and the *Acetum bezoardicum, rubi idaei, sambuci, calendulae*, with red roses, white lillies, pæony, white mustard, saffron, *Theriaca*, and *Mithridate, species Diamargarit. frigid. Diatrageanib. frigid. &c.* and because in compounding such topical medicines, one can hardly be much out of the way, as to their quantity and form, therefore nurses may as well mix them as apothecaries: The common people made use of *Theriaca*, bay berries, bitter almonds, thyme, pepper-wort, water-germander, viperi-grass, &c. mixed with crumbs of bread, and salt, a little camphire, or vinegar, in which was stamped rue, plantain, houseleek, parsley roots, as also leaven, &c.

The third fountain of healing, viz. surgery, afforded a great deal of relief: To mollify the *Bubo's* some made use of cakes of black pepper, mixed with vinegar, and oil of roses, or white lillies; or they mixed these cakes with honey, figs, and wheat-flower, and applied them warm in manner of a poultice; others took camomile flowers, fresh butter and lincticed; to draw the *Bubo's* it was very common to apply roasted onions, roasted figs, or leaven, mixed with vinegar, white mustard-seed, and powder of *Spanish flies*; and then after that, they used a plaister of oil of turpentine, tar, and yellow wax: Some very much recommended *Emplastrum manus Dei le mort*; others the *Emplastrum diachylon cum gummi Vigonis*, by itself, or mixed with the *Oxycroceum*, to mollify the *Bubo's*; some by means of cupping-glasses, or blisters, or even by incisions, extracted them, and afterwards cured themselves; but the carbuncles required more pains, care, and caution; in the beginning an *Emplastrum defensivum*, applied about the carbuncle to prevent its further progress, did much good; afterwards to ease the pain, and bring it to a separation, they pursued it with caustics, scarifications, *unguentum Aegyptiacum, Unguentum Basiliconis simplex*, &c. or what else the surgeon thought

thought proper; at last they applied digestives to cleanse it, and a healing plaister.

The Dr. shews how he instructed those (who either out of poverty could not, or a silly apprehension would not make use of a surgeon) to cure themselves with success, not only of *Bubo's*, but also of *Carbuncles*: He has already observed, that plaisters need not be applied to *Bubo's*, till four or five days after their appearing; he ordered the *Emplastrum Diachylon. composit.* *Oxyroceum* and *Melilot*, of each a like quantity, well mixed; or else the plaister, which at *Dantzick* is called *Voegedingianum*, consisting of *Lithargyrium*, oil and wax; others took *Lithargyrium*, *Minium*, &c. as it is described by *Monconys* in his travels p. 119. If it happened that the *Bubo* dissolved and went away, it was let pass without more ado; but if it grew larger, then Dr. *Gottwald* ordered them to apply a blister of leaven, vinegar, and *Spanish flies*, upon which the *Bubo* would gradually come to break; and then with a digestive made of the yolk of an egg, turpentine, mastic, *Peruvian balsam*, and a little saffron, with the addition of *Lapis medicamentalis Crollii*, it was brought to cleanse; and after the purulent matter was well discharged, the ulcer soon healed by means of the aforesaid plaister: *Carbuncles* were soon cured by the following method; he ordered the part round the *Carbuncle*, as yet untouched, to be well washed with *Aqua calcis*, and then put the *Emplastrum Apostolicum Zwelferi* over the *Carbuncle*; and over that again a poultice clapped between two linnen rags, and applied three times a day, made of *Camomil flowers*, *Lintseed*, *Flower of beans* and *barley*, boiled in milk to a proper consistence; this drew forth the *Carbuncle* admirably well; so that the eschar softened, and began to sweat, and likewise soon came to a separation; at last the ointment, or digestive only, with a defensive plaister cleansed it, and the cure was perfected with *Emplastrum Voegedingianum*; this method had surprising success.

An Account of the Roman Eagles; by Dr. Musgrave. Phil. Trans. N° 337. p. 145. Translated from the Latin.

THO' *Steuvechius* and *Lipsius*, men well versed in the military affairs of the *Romans*, are of opinion, that their eagles consisted entirely either of solid gold or silver; and tho' *Dio Cocceius*, lib. 40. calls them *αἰετες χρυσεαί*, and *Arrian Dissertat. Epicteti*, *εἰκοναὶ χρυσαί*, and again *Xenophon Anab.* speaking of the *Persians*, from whom this military en-

sign

sign was borrowed, denominates them likewise *χρυσ. viâ*, *Cyriped. lib. 8.* yet they seem to have been only laid over with plates of either of these metals: When the senate decreed a throne in the *Curia* at Rome for *Julius Caesar*, called *Suggestum* by *Florus lib. 2. c. 11.* or *Sella* by *Suetonius de Julio Cesare c. 26.* and *Cicero Philippi. 11. 85.* and *de Divinatione 1. 19.* we are not to suppose it to have been all of massy gold; but rather, according to *Buchner Thesaur. eruditionis scholast. in voce Aureus edit. Lips. 1692.* gilded or done over with a great deal of gold; and to confirm this, *Plutarch in Cesare*, and *Dio lib. 44.* call the same *Sella*, *Διπλό χρυσεός*, i. e. gilded or done over with gold: When *Nero* repaired his house, that had been burnt down, and called it *Aurea*, it is not probable, that it consisted of pure gold, but rather of gilded columns, door-posts, windows, &c. *Sueton. in Nerone, c. 31.*

When *Cicero* calls the Roman eagle *Argentea*, *Orat. in Catilinam, c. 9.* it is not probable, that it was all of massy silver; and when *Pliny lib. 33. c. 12.* tells us that *Mithridates's* chariots, in the triumph of *Pompey the Great*, were *aurei & argentei*, it is not to be imagined that they were of pure gold or silver; the reason of the thing itself contradicts this; for, every one must know, that these metals will not bear friction; besides, the weight of gold is such, that a chariot of that metal would be altogether useless, because it could not be drawn: In these and the like expressions, we are to consider, not only the sound of the words, and their primary meaning, but likewise the nature of the thing spoken of; and did we carefully observe this rule in explaining ancient authors, we should find it of considerable advantage.

Having premised this, we may observe in the first place, that the eagle was the military ensign of a whole legion, glittering, and fixed to the end of a long *Hasta* or spear; which being raised aloft by the *Aquilifer* or eagle-bearer, and beheld by the soldiers, especially those of the first cohort (consisting, as appears from *Vegetius lib. 2. c. 6.* of 1105 foot, and 132 horse, in all 1237) might be of use to such a number of men in keeping their ranks, observing their distances, and consequently, in retaining their order of battle; therefore, to answer this purpose, the eagle must needs be large and stand high; and first as to its height, in all those adlocutions, or speeches to the soldiers, commonly represented on Roman coins, especially such as have the *Hasta* of a proper length, fixed in the ground, and the eagle raised perpendicular, the height

height of the eagle and *Hasta* together is wont to be to the height of the *Aquilifer*, as 4 to 3, or nearly in a sesquiter-tian ratio; as may be observed in the coins of *Caius Caesar*, and *Galba* (which the Dr. had of a pretty large size and of brass) and others of *Galba*, *Stewech. Comment. ad lib. 3. Vegetii* p. 271. Of *Adrian* *ibid.* and of *Gordian* *Acta Eruditi. Mensis Maii* 1693; and in a coin of *Otho* (which the Dr. likewise had) representing *Securitas P. R.* now supposing the height of the *Aquilifer* to be six foot (which may be reckoned a proper stature for such an officer) it will follow, that the eagle and *Hasta* together are eight foot high, of which at least the 12th part is to be allowed for the eagle, whose depth will be eight inches, and from this we may conclude its other dimensions, and consequently, its weight; which, if the eagle be of massy silver, will be about 300 pounds *Troy*, if of gold, 600 pounds *Troy*; but where shall we find a standard-bearer able to carry and raise aloft such a huge bulk of gold, or even silver, set on the end of a *Hasta*; sure not any.

Lipsius de Militiâ Romanâ lib. 4. from a passage in *Florus*, who speaking of the *Varian* defeat says, that an *Aquilifer* concealed the eagle in a corner of his belt, that it might not fall into the hands of the enemy, would hence suppose, that the eagle was no large body; but as *Florus* is much mistaken in the other parts of this narration, vide *Ryckii Animadversiones ad Tacitum* p. 33. so no regard at all is to be had to him, when he differs so widely both from other authentic authors, and from the truth, and nature of the thing itself.

Dr. *Musgrave* then supposes, that the *Roman* eagle was made either of wood, cork, leather, or paper, or some such light substance, laid over with plates of silver or gold, with expanded wings of the same plate, and sometimes with thunder under its feet; for such an eagle would answer the purpose: What favoured much the notion of the exceeding smallness of the *Roman* eagle, was the too obscure and erroneous account authors give us of its shrine, as if set therein on the end of a spear: It is true, that in the camp at the head quarters there was a chapel or temple, wherein the eagles and other military ensigns were religiously kept and adored, as is evident from *Herodian lib. 4.* and not only so, but there were also such chapels in private houses, as appears from *Cicero Orat. in Catalinam c. 9.*

Upon considering all this, none seems to the Dr. to have explained the matter better than *Ant. Augustinus Dial. III. Antiq. in Cappadociâ*, who writes to the following purpose; 'each

‘ legion carried a carved eagle on the end of a spear or *Hasta*, and incrustated or laid over with plates of gold or silver.’

Plate I. Fig. 1. A coin of *Galba's* representing his effigies on one side, with this inscription IMP. SER. SVLP. GALBA. CAES. AVG. TR. POT. on the reverse, the *Adlocutio*.

Fig. 2. A coin of *Hadrian's*, on one side HADRIANUS. AVG. COS. III. P. P. on the reverse, the *Adlocutio*.

Fig. 3. A coin of *Gordian's* on one side IMP. GORDIANVS. PIVS. FELIX. AVG. on the reverse, the *Adlocutio*.

The three following are brass coins, viz.

Fig. 4. A coin of *C. Caesar*, on one side C. CAESAR AVG. GERMANICVS PON. M. T. R. PO. on the reverse, the *Adlocutio*.

Fig. 5. Another coin of *Galba's*, on the one side SER. GALBA. IMP. CAES. AVG. on the reverse, the *Adlocutio*.

Fig. 6. A coin of *Otho*, on one side IMP. OTHO. CAES. AVG. TR. POT. on the reverse SECURITAS. P. R.

It is observable, that in all these coins, the eagles are nearly of the same height; viz. about three foot above the head of the *Aquilifer*.

An Experiment on the proportions of the ascent of Spirits of Wine, between two glass Planes, whose surfaces were placed at certain different distances from each other; by Mr. Francis Hauksbee. Phil. Trans. N^o 337. p. 151.

MR. *Hauksbee* took two glass planes, about six inches long, and two inches broad, and separated them, after being made clean, at each extremity by 32 pieces of brass plate, whole thickness, when laid upon each other, and pressed together by screws made a distance between the planes nearly equal to $\frac{1}{6}$ of an inch; being thus prepared, he plunged one extremity into some tinged spirits of wine; and after he had wetted the inner surfaces of the planes therewith, by declining the upper extremity, he set them upright, and found that the surface of the spirits between them remained higher than that of the spirits on their outsides, by a distance equal to the first line CD Plate I. Fig. 7. above AB (the line AB being the common surface) in all the experiments, on the outside of the planes; after this, he reduced the distances of the planes to half the former, by taking away 16 of the brass plates from each extremity thereof; then plunging them into the spirits, and using them in all respects as before, he found the spirits stand between the planes just double the height they stood at in the former experiment, as may be observ'd by the line EF, in the figure; again removing half the number of

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Fig. I.

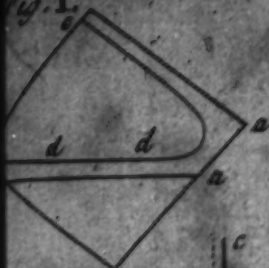


Fig. II.

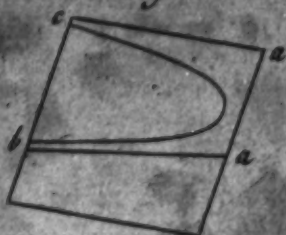


Fig. III.

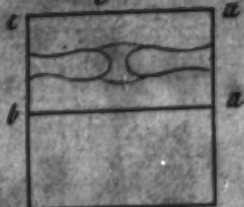


Fig. V.



Fig. IV.

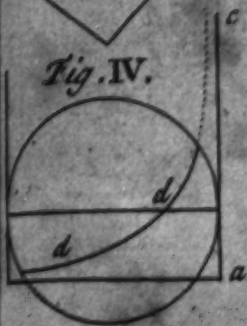


Fig. VIII.



Fig. IX.



Fig. X.

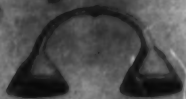


Fig. XI.



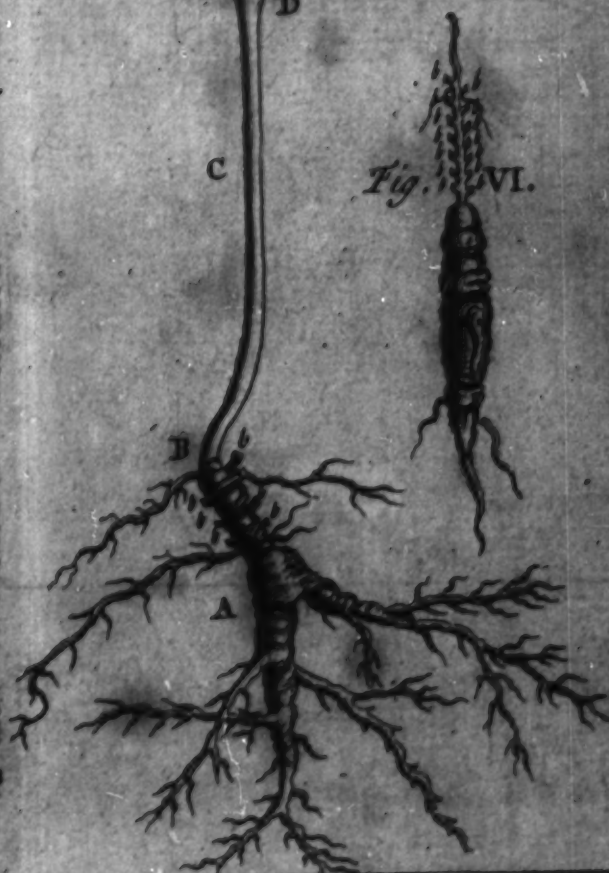
Fig. XII.



Fig. VII.



Fig. VI.



of brass pieces, whereby only eight of them were left, at each extremity, and using them as before, the spirits rose between them to a height just double to that in the preceeding experiment, mark'd in the figure by the line GH; thus from eight he reduc'd them to four pieces at each extremity of the planes; then again the spirits were observ'd to remain suspended at twice the last mentioned height, represented by the line IK; in this last experiment the planes were distant from each other but $1\frac{1}{2}$ of an inch; nearer than that he could take no certain measure; but he supposes, that the above mentioned experiments are sufficient to ground a calculation upon, even of the nearest approximations.

Some further Experiments, shewing the ascent of Water between two glass Planes in an hyperbolic Curve; by the Same. Phil. Trans. N^o 337. p. 153.

MR. Hawksbee found by several experiments, that the hyperbolic curve, mentioned *Phil. Trans.* N^o 336. holds in all directions of the planes, the asymptotes being always, the one the surface of the water, and the other a line drawn along the touching sides; thus when the touching sides were plunged under the surface of the water, and the angle c depressed, and made to remain lower than the angle a Plate I. Fig. 8. there would then be produced such an oblique curve, as shewn by the Fig. in which and in the Fig. 9, 10, 11, and 1, 2, 3, 4, Pl. II. ab represents the surface of the water on the outside of the planes, and ac their touching sides; now tho' the curve dd rise between the planes in such an obliquity, yet it conforms itself in its figure to the asymptotes, viz. ab the surface of the water, and ac the touching sides of the planes; for, supposing the asymptote ac to be continued, as in the pricked line, till it surmount the surface of the water to such a height; or suppose the planes extended in the same manner; then would the water remain between them in the appearance of the pricked lines, being at all distances from the axis of the curve, equal in respect to the asymptotes, and so of all the rest of the curves, which are the result of the several angles, made by the touching sides of the planes: Now when the touching sides were placed upwards, parallel to the surface of the water, as in Fig. 3. Pl. II. and quite plunged under it, then upon lifting them up in the same position, till the weight of the water between the planes overbalanced the power of their attraction, two curves, one from each side of the planes, would open themselves, and meet each other in the middle, as represented in the aforesaid figure; where they would unite,

unite, and form a figure, as joined by the pricked lines, being wider in the middle than towards the sides of the planes; and it is very remarkable, that this curve would always break out between the planes, at an equal distance between the touching sides, and the surface of the water: The same figure is likewise produced between two round glass planes, as represented by Fig. 4. the asymptotes being the same as the former; that is, one the surface of the water, the other a tangent drawn from the touching point, parallel to a tangent drawn from the open or opposite part of the planes, at right angles with a line drawn thro' the axis thereof: Mr. Hauksbee found these experiments answer the same in *Vacuo*, as in the open air; so that that medium has nothing to do in this extraordinary *Phænomenon*: The planes, made use of in the foregoing experiments, were about seven inches square, opened on one side to an angle of 20 minutes, or thereabouts; the round planes were nearly three inches in diameter,

A farther Account of the ascent of drops of Spirits of Wine between two Glass Planes 20 Inches and a half long, with a Table of the Distances from the touching extremities, and the Angles of Elevation; by the Same. Phil. Trans. N^o 337. P. 155.

THE spirits of wine did not move so nimbly between the planes, as oil of oranges, which gave Mr. Hauksbee an opportunity of observing the angles with more deliberation; the limb, on which the planes were laid, mov'd in the centre of a quadrant of four foot radius, the largeness of which gave him opportunity of measuring the angles with greater accuracy; but the distance between the drop on the planes, and the graduations on the quadrant made it somewhat difficult to observe them both at the same time; yet the following tables may be depended on to be as true as the nature of such an experiment is capable of: In *Phil. Trans. N^o 334*, there is a particular account of the manner of making this experiment: The tables are calculated from the touching extremities of the planes; and it is to be observ'd, that in the table where the planes were opened but to an angle of 10', that he could not come nearer than four inches of the touching extremities in his observations; but so far as he could go, it seemed to be much in the same proportion (as he often observ'd in the course of these experiments) with the table where the planes were opened to an angle of 18'.

Distance

Distances in inches from the touching extremity. The planes open- ed to an angle of 18°.	Angle of ele- vation, D. M.	Distances in inches from the touching extremity. The planes opened to an angle of 10°.	Angle of ele- vation, D. M.
18 $\frac{1}{2}$	0 45	2 $\frac{1}{2}$	18 50
16 $\frac{1}{2}$	0 55	2 $\frac{3}{4}$	23 25
14 $\frac{1}{2}$	1 05	2	30 00
12 $\frac{1}{2}$	1 20		
10 $\frac{1}{2}$	1 30		
9 $\frac{1}{2}$	1 40	18 $\frac{1}{2}$	1 30
8 $\frac{1}{2}$	2 00	16 $\frac{1}{2}$	1 50
7 $\frac{1}{2}$	2 30	14 $\frac{1}{2}$	2 10
6 $\frac{1}{2}$	3 20	12 $\frac{1}{2}$	2 40
5 $\frac{1}{2}$	4 25	10 $\frac{1}{2}$	3 10
4 $\frac{1}{2}$	6 00	9 $\frac{1}{2}$	3 30
4	7 23	8 $\frac{1}{2}$	4 00
3 $\frac{1}{2}$	8 40	7 $\frac{1}{2}$	5 05
3 $\frac{1}{4}$	9 25	6 $\frac{1}{2}$	7 40
3 $\frac{1}{8}$	10 30	5 $\frac{1}{2}$	10 50
3	12 40	4 $\frac{1}{2}$	14 00
2 $\frac{1}{2}$	15 00	4	18 00

Some further Microscopical Observations on the Animalcula found upon Duckweed, &c. by M. Leewenhoeck. Phil. Trans. N° 337. p. 160.

IN *Phil. Trans.* N° 295. M. Leewenhoeck took notice of the surprising figure of an *Animalculum*, that was fix'd in a little scabbard, or sheath, fastened to some of the small green weeds, found in ditches full of water; and as often as he view'd these *Animalcula*, and shew'd them to others, they could never be satisfied with looking on such surprising objects, and the more so, as they could not conceive how so odd a motion, as all of them had, could be performed; as also what could be the use of such a motion; for, when we observe other animals, that are endu'd with motion, moving any part of their bodies, we presently conclude, that these parts are not formed in vain; and consequently, we may conclude, that the wheel-like motion of the above-mentioned *Animalcula* is useful to their bodies, tho' we cannot tell exactly how.

In the latter end of *July*, and beginning of *August*, M. Leewenhoeck caus'd some of the green weeds, commonly call'd duckweed,

weed to be taken out of the water, that runs with a gentle current thro' *Delft*, in order to observe the above-mentioned *Animalcula*, together with others of several sorts, that were fastened to the duckweed, or ran about upon it: Amongst others, he found some *Animalcula*, whose sheaths, at the extreme parts, were a little thicker than the hair of ones head, compos'd of small globules very easily distinguishable: He view'd one of these *Animalcula* for a good while together, and observ'd several times one after another, that when the *Animalculum* thrust its body out of the sheath, or case, and the wheel-like, or indented particles mov'd at the same time in a circle, there appeared out of a clear and transparent place, a small round particle, which, without nice viewing it, could hardly be perceived; and this particle, growing larger, mov'd very swiftly about its axis, as it were, and continued without any alteration in its place, till the *Animalculum* had drawn back part of its body into its sheath; in doing which it placed the said round particle on the edge of its sheath, which thus became augmented with a round globule; and whereas the *Animalculum* had placed the said globule on the east part of its sheath, at another time it fix'd it upon the south, or north side, by which means the sheath was regularly increas'd on all sides; whence it plainly appears, how much the divine wisdom is display'd in the regular formation of the said *Animalcula*.

Having further and very accurately view'd the circulating indented wheel-work, he observed that it caus'd an exceeding great motion in the water about it; by which means several very minute particles (only visible thro' the microscope) were waisted to the said *Animalculum*, and others driven away; the *Animalculum* made use of some of these particles, that were thus drawn to it by its circulating instrument for food and nourishment, and other particles that were almost come to it, were driven away very swiftly, and rejected, as it were, by the *Animalculum*; whence M. *Leewenhoeck* inferr'd, that those particles, which were thus thrust away, were not proper for its food: From this discovery we may conclude, that seeing this kind of *Animalculum* cannot move from place to place in the water, nor consequently go in quest of its food, as other animals do, that are endu'd with motion, being fastened by the tail, or other parts of the body, it must necessarily be provided with such instruments as are fit to move the water, and by that means come at the particles floating therein, which serve for the nourishment, increase, and defence of its body: If we observe those *Animalcula*, which with their long tails are fastened to some part, or other of the weeds,

weeds, as a great many are discover'd on the little roots of the duckweed, we may see, that they do not only make a circular motion with the extreme part of their bodies, which motion, in proportion to the said part, is very great; but that they likewise can draw in their tails, and that with a very quick motion; by which means they can move the water out of its place, when they stretch their tails out again, and thus bringing fresh water under them, they procure new food: M. *Leewenboeck* likewise observ'd a very few *Animalcula*, whose bodies were short and thick, and much larger than those other *Animalcula* that lodged themselves in a sheath, and were fastened by their tails, or extreme parts of the small roots of the duckweed; and tho' these short, and thick *Animalcula* could move from place to place, yet they also had a circular motion in the fore part of their bodies; whence he concluded, that those motions serv'd some other purposes than only to draw in their food; upon farther considering what might be the use of these indented wheel-works, which resemble so much the indented wheels of a clock, or watch, he owns that they are very necessary to produce a great motion in the water; for were it a round, and smooth wheel, it would produce but a very small motion; whereas now, each tooth in the said wheel, or circle produces a considerable motion, in comparison of what a smooth and plain one would do; whence appears the surprising order, observ'd in the formation of such *Animalcula*, which are not to be perceived with the naked eye.

In the beginning of *August* M. *Leewenboeck* being in a garden where was a pond full of fish, observ'd upon the water of the said pond a thin scum floating, that was of a greenish colour, tho' there was nothing green in the water; which seemed surprising to him, because he had observ'd at other times, that the water of the said fish-pond was very clear, and so was that whence it was supplied; and he was told, that when it rained, the said scum disappear'd; he took a little piece of wooden lath, and drawing it over the surface of the water, he placed a small drop of the water upon a green wine-glass, and viewing it with the microscope, he discover'd a vast many *Animalcula* so exceeding minute, that they almost escaped his sight thro' the microscope; there were also several sorts of larger *Animalcula*, mixed with a great number of little air bubbles of an exceeding smallness: Now when people wash their beer and wine glasses in such a pond, who can tell how many of those *Animalcula* may remain sticking upon the sides thereof, and by that means be conveyed into the mouth; which being so, it appears, how the *Animalcula*
M. Lee-

M. *Laewenboeck* observed in the matter that remains in and about our teeth, were conveyed thither.

M. *Laewenboeck* observed in the flesh of a whale (and the same is to be said of an ox, and even down to a mouse) that its fibres, tho' 16 times smaller than a thick hair of his beard, yet notwithstanding are surrounded, or involved in little membranes, in such a manner, that the fleshy particles do not touch each other.

A Woman big with Child, who recovering of the Small-pox, was afterwards delivered of a Dead Child full of the pustules of that Distemper; by Mr. Derham. Phil. Trans. N^o 357. p. 165.

A Certain woman with child had the small-pox, and was pretty well recovered, so that she was able to take something to purge her after them; and on the 30th of *August* she took a purge, which did not work; and on the 1st of *September* another purge, which only gave her a stool or two; upon which on the 3d of *September* she took another stronger purge, that worked so violently upwards and downwards, that she fell into faintings, and convulsions; about which time Mr. *Derham* supposes her child died, of which she was not delivered till the 8th of *September*; the child was a female, and in appearance well made, lusty, and strong; at its delivery the midwife judged it had been dead five or six days; so that its belly was burst, and the bowels came out; and the whole body tended to putrefaction: But what seemed most remarkable to Mr. *Derham* was, that the child was so very full of the small pox, that hardly a pin's head could be put between the blisters, which were very plump, and full of matter, like the pustules of an adult, when the small-pox are at the height, only a little depressed in the middle; but as full of pustules as the child was, the mother had but few and very favourably; the child, as Mr. *Derham* supposes, undergoing that which would have been more severe upon the mother.

From this account of the above-mentioned case Mr. *Derham* queries. 1. When a woman in pregnancy hath the small-pox, whether it be likely that the child should be in danger of catching and having that distemper after its birth? 2. Whether the above-mentioned child had the small-pox at the very same time with the mother, and not rather afterwards, because the child was full of them, after the mother was well recovered?

Mr. *Der-*

Mr. *Derham* is apt to think, that the great flux, and tendency of the blood to the child in the womb might draw in the humour, and prevent the greater eruption of the small-pox in the mother; and that for want of a proper expence thereof, the remainder broke out afterwards in the child, and that it really had them after the mother; nature making the discharges upon the child, which were not completed on the mother.

Several Observations made in Natural History at North-Bierley in Yorkshire; by Dr. Richardson. Phil. Trans. N^o 337. p. 167.

ONE *John Worsnape* of *North-Bierley*, who lived till he was 17 years of age, never made water, and yet was in perfect health, vigorous, and active; he had constantly a *Diarrhæa* upon him, but without much uneasiness; the obstruction must have been in his kidneys, for, he never had any inclination to make water; the serous part of the blood, which should have been thrown off by urine, was discharged by the coeliac and mesenteric arteries, by means of the glands into the guts; he died of a fever: The second instance is of old age in *Martha Waterhouse* and *Hester Jagar* both born in *North-Bierley*, and own sisters; the former, who had been married, died in 1711, in the 140th year of her age; and the latter died unmarried in 1713 in the 107th year of her age; the Dr. would not have taken notice of their age separately, it falling so far short of several instances, which have happened in *Yorkshire*, but jointly there is not any that comes up to them.

On *Monday* towards night great quantities of tench being taken in a pond, belonging to one Mr. *Tempest* of *Broughton in Craven*, and some of them being brought to *Bradford* on *Tuesday* about the same time, on *Wednesday* six of them were sent the Dr. Upon opening the basket, wherein they lay, which was set upon a table not far from a good fire, and had remained there till *Thursday* morning, he thought the eyes of some of them looked clear, and putting two of them into a pail of water, in less than two hours time they swam very lively therein; the other four having no signs of life, he put them into the same pail, and before night they all swam about in it: *Burboles*, being a fish not frequently met with in the southern rivers of *England*, are often found in this county, especially in slow rivers, and standing waters; as in the river *Foss* in *York*, and also in *Derwent*; but no where more frequent, than in the fen-ditches of the levels, about four miles from *Doncaster*: The Dr. has seen several times abundance of small trouts caught by angling in the moun-

tainous lakes of north *Wales*, notwithstanding the difficult access to these places, where a good footman can scarce climb up to them; the Dr. does not doubt, but that these lakes are yearly supplied from the brooks at the bottom of the mountains, especially in spawning time, when the trouts endeavour to surmount all difficulties, by passing up the small rivulets to deposite their spawn for the preservation of their species, where it is the most secure from the violence of other fish, and by accident fall into these natural ponds, where they continue all summer; not only the trouts, taken in these mountainous lakes, are small, but likewise the charra, taken as they ascend the small river out of the large lakes near *Lhan Berys*, to deposite their spawn in the sands there; these very rarely exceed a fresh herring in bigness, as the Dr. was assured by Mr. *Evans* vicar of *Lhan Berys*, who saw several of them caught; and by his account of them they are no ways different from those taken in *Winander-Mear*, excepting in magnitude, where it is no unusual thing to meet with them of two pound weight, and upwards; this smallness in fish the Dr. some time imagined to be owing to the coldness of the water, these lakes being supplied with fresh water from the mountains for eight months in twelve; the *Minera* of vitriol and allum, being often met with in the hills thro' which some of the water must drain, does, perhaps, not a little contribute to the roughness, and coldness of the water; the contrary effects are found in waters that run thro' the limestone rocks, where no rough salts are found; the trouts there are large and fat; an instance of which we find in the trouts in *Malham Tarr* in *Craven*, near *Settle*, where they are frequently found two foot long.

The Dr. corrects a mistake in Mr. *Ray's Synopsis quadruped. Esc.* p. 195. where he says that *Mustela vulgaris* is called here a *Foumart* or *Fitchet*; *Putorius* is called in the north of *England*, a *Foumart*, quasi foul mart, or stinking mart, in opposition to the marts that emit a musky smell, and are often met with in the woods there, and taken by the hunters in snows: The ermine is frequently met with there in winter, and looked upon, when they appear, to presage snow; it begins to change its colour from brown to white, about the beginning of *November*; he had seen one or two of them, that in the beginning of *March* were changing from white to brown. *Qu.* whether these animals do not always continue white in the more northern parts of the world?

The *Nut-hatch*, or *Nut-jobber* is not frequently to be met with in the south of *England*, yet is so common in the north,

that the Dr. has sometimes seen six or seven of them in one day in the woods; and he thinks this must be the bird Dr. Plot in his *Nat. Hist. of Oxfordshire* calls a wood-cracker, and which he takes to be undescribed; he has with much pleasure often observed these birds crack nuts, which they do with very great dexterity; and a servant of his, observing about *Christmas* whence she brought her provisions, soon discovered it had been from a hollow tree, and cutting the place open, he brought from thence several pints of very choice nuts: The Dr. met with a nest of the *Regulus Cristatus* in a thick thorn hedge in his orchard, built round with a little hole at the side; the outside was green moss, the inside hair, and feathers, not much unlike that of the common wren; the eggs were small, and white, with several brown spots upon them; the note of the cock is very agreeable, not much unlike some of the *Parus* kind; the Dr. never met with the *Coclea promatias major Gesneri* in the north; but he found it plentifully about the middle of *May* in *Stunsfield* fields amongst the briars and brakes, especially near the famous *Roman pavement*.

Some Observations on the mechanic Arts, and Physic of the Indians in 1709; by F. Papin. Phil. Transl. N° 337, p. 225.

THE *East-Indies* furnish more materials for mechanic arts and sciences, than any other country in the world; the artificers there have a surprising skill, and dexterity; they excel particularly in making linen cloth, which is so fine, that very long, and broad pieces thereof may easily be drawn thro' a small ring: If you tear a piece of muslin into two, and give it to one of their fine drawers to set it together again, it will be impossible for you to discover where it is joined, tho' you mark it on purpose to know it; they will place together pieces of glass, or *China-ware* so artificially, that one cannot perceive it was ever broken: Their embroiderers work very curiously in filigreen; they imitate exactly any work made in *Europe*, tho' the machine they make use of, and all their other utensils, do not cost them upwards of a crown; the looms their weavers use, cost no more; with these they sit in their courts, and yards, or on the side of the highway, and work those fine stuffs so highly esteemed over all the world: Here they have no need of wine to make *Aqua vite*; but make it of a syrup, sugar, some certain sorts of barks, and raisins; it burns better, and is stronger than any made in *Europe*. They paint flowers, and gild very finely upon glass; F. Papin was surpris'd to see the vessels, they use

to cool water in, no thicker than two leaves of paper pasted together: Their watermen row after a different manner from ours in *Europe*; they move the oar with their feet, and their hands serve instead of the *Hypomochlion*, or roller on which it turns: The liquor, their painters use, does no ways lose its colour, nor is it tarnished by lee: The husbandmen in *Europe* prick their oxen with a goad to make them go the faster; but here they only twist, or wring their tails; these beasts are very docile; they teach them to lie down, and rise up, when they either take up, or lay down their burden: They make use of a hand-mill to break their sugar-canes, which does not cost them above 10 pence; the person that grinds, works and fashions the stone with lac and emery: Their masons will pave the largest room with a sort of cement made of brick-dust, and lime; so that it shall seem to be but one stone, and is much harder than gravel: *F. Papin* saw them make a sort of pent-house 40 foot long, 8 foot broad, and 5 or 6 inches thick, which they raised up, and fixed to the wall on one side only, without putting any prop underneath to support it: Their pilots take the altitude, or latitude of places with a cord that has several knots in it; they put one end of the cord between their teeth, and by means of a piece of wood fixed to it, that has a hole thro' it, they easily observe the tail of *Ursa minor*, which is commonly called the polar-star, or north-pole: Their lime is usually made of sea-shells; that made of snail-shells serves to whiten their houses, and that made of stones they chew, with the leaves of *Betel*; *F. Papin* has observed some of them that would take the quantity of an egg in a day: They make their butter in the first pot that comes to hand; they cleave a stick at one end into four quarters, and stretching them out asunder in proportion to the size of the pot, that contains the milk, they turn the stick round different ways, backwards and forwards, by means of a cord twisted about it, and by this means make the butter in a short time: Such as sell butter have the art of making it pass for fresh, when it is old and rank; to do this, they melt it, and pour upon it sour curdly milk, and in eight hours after they take it out in lumps, and strain it thro' a cloth for sale: Their chemists make use of the first pot they meet with to revive cinabar, and other preparations of mercury, which they do in a very simple manner; they easily reduce all metals into a powder, and which *F. Papin* saw them do; they put a great value upon talc, and brass, which consume as they say, all viscous humours, and remove the most stubborn obstructions: Their physicians are

are more cautious in using sulphur than they are in *Europe*; they correct it with butter, and put broth upon it made with long pepper, wherein are boiled the kernels of the *Indian* pineapple: In fevers they use with success wolfs-bane corrected in cow's urine, and arsenic corrected with juice of lemons: A physician is not permitted to take care of a sick person, unless he can guess at his disease, and what humour is most predominant, which they easily know by feeling the patient's pulse, and they are seldom deceived: The principal diseases that reign in this country are, 1. The *Mordechin* or *Colera morbus*; to cure it, they forbid the patient to drink, and burn the soles of his feet. 2. The *Sonipar*, or lethargy, which is cured, by putting into the patient's eyes bruised pepper, mixed with vinegar. 3. The *Pilhai*, or obstruction of the spleen, for which they have no specific remedy, unless it be that of the *Joghis*, or converted *Indians*; they make a small incision under the spleen, and put in a long needle between the skin and flesh, from whence, by sucking with the end of a horn, they extract out of the orifice a kind of fat matter, resembling *Pus*: Most of the physicians have a custom of putting a drop of oil on the patient's urine; if it spreads abroad, they say it is a sign, that the patient is very hot within; but on the contrary, if it keeps together entire, it is a sign he wants heat: The common people use very simple medicines; for the megrim, they smoke, like tobacco, the dried bark of a pomegranate tree, reduced to a powder, and mixed with four corns of pepper; for the common head-ach, they smell to a nodule, composed of a mixture of *Sal armoniac*, lime, and water, tied up together in a linen rag; they cure such dizzinesses of the head, as proceed from a cold thick blood, by drinking wine, in which are steeped a few grains of frankincense; for deafness, occasioned by too great a quantity of cold humours, they drop into the ear a drop of the juice of lemons: When the brain is charged and oppressed with watery humours, they smell to black cummin-seed, bruised and tied up in a nodule: For the tooth-ach they put upon the tooth affected a paste made of crumbs of bread, and the seed of the *Stramonium*, which stupifies the part affected, and eases the pain: In an hemorrhage, or flux of blood they make the patient smell to bruised motherwort, or wormwood: For too great a heat of the breast, and spitting of blood they cover over with paste a *Giraumont* (an *Indian* fruit like a gourd, and which tastes like a *Citrus*) which they bake in an oven, and drink the water that comes from it: For the colic, that proceeds either from wind, or watry humours, they

they give to drink four Spoonfuls of water, in which anniseeds and a little pepper have been-boiled till the half is evaporated; they also bruise an onion with ginger, and apply it to that part of the belly, where there is most pain: For the lientery they roast a clove of garlick under the ashes, and when they go to bed they hold it in the mouth, and suck out the juice of it; if they drink the juice of the leaves of cucumber bruised, it purges and vomits them; they cure a difficulty of urine, by drinking a spoonful of oil of olives, well mixed with a like quantity of water: For a looseness they torrify a spoonful of white cummin seed, and a little powdered ginger, which they swallow mixed with sugar: F. Papin has seen them cure fevers, which begin with a shivering fit, by giving the patient three large pills, made of ginger, black-cummin, and long pepper: For tertian agues they give the patient for three days together three spoonfuls of the juice of teucrium, or great germander, with a little salt and ginger.

An Account of the luminous Appearance observable in the Wake of Ships, in the Indian Seas; by F. De Bourzes.
Phil. Trans. N^o 337. P. 230.

AS to this luminous appearance in the wake of ships, F. De Bourzes observes. 1. That when the ship ran apace, a considerable light was often observed therein; such as did not view it narrowly, often attributed it to the moon, the stars, or the lanthorn at the stern, as F. De Bourzes himself did, when he first perceived it; but having a window that looked directly down upon it, he was soon undeceived; especially, when he saw it appear more bright, the moon being under the horizon, the stars cover'd with clouds, and no lights in the lanthorn, or any other light whatsoever cast upon the surface of the water. 2. That this light was not always equal, some days very little, other days none at all; sometimes brighter, at other times fainter; sometimes again, very vivid, and at other times, nothing at all of it to be seen. 3. That as to its brightness, F. De Bourzes could easily read by it, tho' he was 9 or 10 foot above it from the surface of the water; as he did, particularly on the 12th of June and the 10th of July 1704, the title of his book, which was in large characters. 4. That as to the extent of this light, sometimes all the wake appeared luminous to 30 or 40 foot distance from the ship; but that at any considerable distance, the light was very faint. 5. That some days one might easily distinguish in the wake, such particles as were luminous from those that were not so; that at other times there was no difference; and that then the wake resembled

a river of milk, and afforded a very agreeable sight, particularly as it appeared on the 10th of *July* 1704. 6. That at such times as the bright parts could be distinguished from the others, they were observed not to be all of the same figure; some of them appearing like points of light, others almost as large as stars appear to the naked eye; some looked like globules, of a line or two in diameter, and others like globes as big as one's head; oftentimes these *Phosphori* formed themselves into squares, of three or four inches long, and one or two inches broad; sometimes all these different figures were seen at the same time; and particularly on the 12th of *June* the wake of the vessel was full of large vortices of light, and these oblong squares, above-mentioned; another day when the ship sailed slowly, the vortices appeared, and immediately disappeared again, like flashes of lightning. 7. That not only the wake of a ship produces this light, but likewise fish in swimming leave behind them a luminous tract, which is so bright, that one may distinguish the largeness of the fish, and know of what species it is; *F. De Bourzes* has sometimes seen a great many fish playing in the sea, which have made a kind of artificial fire in the water, that yielded a very pleasant sight; and oftentimes only a rope placed across the water will break it in such a manner, that it will become luminous. 8. That if one take some water out of the sea, and stir it in the dark never so little with his hand, he may observe in it a vast many bright particles. 9. That if one dip a piece of linnen in sea water, and twist or wring it in a dark place, he may observe the same thing; and if he do so, tho' half dry, yet it will produce a great many bright sparks. 10. That when one of the sparkles is once formed, it continues a long time; and if it fix upon any thing that is solid, as for instance on the side, or edge of a vessel, it will continue shining for some hours together. 11. That this light does not appear at all times, tho' the sea be in great agitation; nor does it always happen, when the ship sails fastest; neither is it the simple beating of the waves against each other that produces this brightness, as far as he could perceive; but he has observed, that the beating of the waves against the shore has sometimes produced it in great abundance; and on the coast of *Brazil* the shore was one night so very bright, that it appeared, as if it had been all on fire. 12. That the production of this light depends very much on the quality of the water; and *F. De Bourzes* supposes, that generally speaking, other circumstances being equal, the light is largest, when the water is faintest, and fullest of foam; for, in the main sea the water is not every

every where equally pure; and sometimes if one dip linnen into the sea, it feels clammy when drawn up again; and he has often observed, that when the wake of the ship was brightest, the water was more fat and glutinous, and linnen moistened therewith produced a great deal of light, if stirred or moved briskly. 13. That besides, in sailing over some places of the sea, they find a substance of different colours, sometimes red, sometimes yellow; in looking at it one would take it for saw-dust; but the sailors affirm that it is the spawn or seed of whales; what it is, is uncertain; but when they draw up water in passing over these places, it is always viscous, and glutinous; the mariners also say, that there are a great many heaps, or banks of this spawn in the north; and that sometimes in the night, they appear all over of a bright light, without being put into motion by any vessel, or fish passing by them. 14. But to confirm farther what has been said, viz. That the water, the more glutinous it is, the more it is disposed to become luminous, F. De Bourzes adds one particular he saw himself; one day they took a fish, the inside of whose mouth appeared in the night like a burning coal; so that without any other light, he could read by it the same characters he had read by the light in the wake of the ship; its mouth being full of a viscous humour, they rubbed a piece of wood with it, which immediately became all over luminous; but as soon as the moisture was evaporated, the light was extinguished.

As to the marine rainbows, F. De Bourzes observed one after a great tempest off the Cape of Good Hope; the sea was very much agitated, and the wind sweeping the tops of the waves, formed a kind of rain, in which the rays of the sun painted the colours of a rain-bow: It is true, that the common *Iris* has this advantage over the former, that its colours are more lively, distinct, and of a longer extent; in the marine *Iris* only two colours could be distinguished, viz. a dark yellow on that side next the sun, and a pale green on the opposite side, the other colours being too faint to be distinguished; but in recompence for this, their *Iris*'s are in greater numbers; one may see 20 or 30 of them together; they appear at noon-day, and in a position opposite to that of the common rainbow, that is to say, their curve is turned towards the bottom of the sea, as it were.

As to exhalations in the night they form a long tract of light in the air, much larger in the Indies than in Europe; F. De Bourzes had seen two or three, that he would have taken for real rockets; they appeared near the earth, and cast a light like that of the moon, some days after her change; they fall slowly, and in their

their descent form a curve line, especially one he saw on the main ocean, at a great distance off at sea, on the coast of *Malabar*.

An Account of a Woman who had her Menfes regularly to the 70th Year of her Age; by Mr. James Yonge. Phil. Trans. N^o 337. P. 236.

THERE died at *Lamerton*, 15 miles from *Plymouth*, a woman of 86 years of age, who to the 70th year had her *Menfes* plentiful and regular; at that time they ceased, and soon after there ensued the like efflux from the hemorrhoids, which continued, till she got beyond fourscore; she was till then healthful, and strong, of a vigorous aspect, smooth, plump, and florid in countenance, like one not half so old; her appetite was very good; her intellectuals clear, and sound; and her sight so perfect, that she could to the last thread a needle, and read small print without glasses; when that flux ceased, she became gouty; and about a year before she died, there arose an aposthumation on one of her wrists, which opened, and discharged a deal of chalky matter, and some stones; the day she died, she arose out of bed, and after performing some christian devotions, expired: She was never sick, before the stoppage of the hemorrhoidal flux, excepting once at *Exeter*, where she was born, and then lived, she became infected with what they called the plague; it terminated in a critical abscess in one of the emunctories; and which was very surprising, during all the time of that sickness, she nursed a male child.

The Description of a Tartarian Plant, called Gin-seng, with an Account of its Virtues; by F. Jartoux. Phil. Trans. N^o 331. P. 237.

THE most eminent physicians in *China*, have writ whole volumes on the virtues and qualities of the famous plant *Gin-seng*, and make it an ingredient in almost all medicines, which they give to their chief Nobility; for, it is of too high a price for the common people; they affirm, that it is a sovereign remedy for all weaknesses, occasioned by excessive fatigues either of body or mind; that it dissolves pituitous humours; that it cures weakness of the lungs and the pleurisy; that it stops vomitings; that it strengthens the stomach, and helps the appetite; that it disperses fumes, or vapours; that it fortifies the breast, and is a remedy for short, or weak breathing; that it strengthens the vital spirits, and encreases the *Lympha* in the blood; in short,

that it is good against dizziness in the head, and dimness of sight, and that it prolongs life in old age: No body can imagine, that the *Chinese* and *Tartars* would set so high a value upon this root if it did not constantly produce a good effect; those that are in health often make use of it to render themselves more vigorous and strong; and *F. Jartoux* is persuaded, that it would prove an excellent medicine in the hands of any *European*, who understands pharmacy, if he had but a sufficient quantity of it to make such trials as are necessary to examine its nature chemically, and apply it in a proper quantity according to the nature of the disease, for which it may be beneficial. It is certain, that it subtilizes, warms and increases the motion of the blood; that it helps digestion, and invigorates in a very sensible manner; and *F. Jartoux* found several times from experience, that the root taken raw and unprepared had the above-mentioned effects upon himself; he also observed, that the green leaves, and especially their fibrous parts chewed, would nearly produce the same effect; the *Tartars* often give a decoction of the leaves of *Gin-seng* instead of tea; it is of a grateful colour; and when one has taken it twice or thrice, its taste and smell become very pleasant: As for the root of this plant, it is necessary to boil it a little more than tea, in order to extract its virtue; as is practised by the *Chinese*, when they give it to sick persons, on which occasion they seldom use more than the fifth part of an ounce of the dried root; but as for such as are in health, and take it only for prevention, or some slight indisposition, *F. Jartoux* would advise them not to make less than 10 doses of an ounce, and not to take it every day: It is prepared in the following manner, the root is to be cut into thin slices, and put into an earthen pot well glazed, and filled with about a quarter of a pint of water, *Paris* measure; the pot must be well covered and set to boil over a gentle fire; and when the water is evaporated to the quantity of a spoonful, a little sugar is to be mixed therewith, and it is to be drank immediately; after this as much more water is to be put into the pot upon the remainder, and to be boiled as before, in order to extract all the juice, and what remains of the spirituous part of the root; these two doses are to be taken the one in the morning, and the other at night: As to the places where this root grows, *F. Jartoux* only observes in general, that it is between the 39 and 47th degree of northern latitude, and between the 10th and 20th degree of eastern longitude, reckoning from the meridian of *Peking*; there is a long tract of mountains, which the thick forests that cover and encompass them render almost unpassable; the *Gin-seng*

Jeng is to be found upon the declivities of these mountains, and in these thick forests, and amidst a great many other different sorts of plants; it is not to be met with in plains, valleys, marshes, the bottoms of rivulets, or in places too much exposed and open; if the forest take fire and be consumed, this plant does not appear till two or three years after; it also lies hid from the sun as much as possible, which shews that heat is noxious to it; all which makes F. *Jartoux* apt to think, that if it is to be found in any other country in the world, it may particularly in *Canada*, where the forests and mountains, according to the relation of such as have lived there, very much resemble these here: The places where the *Gin-seng* grows are separated on every side from the province of *Quan-tong* (in old maps called *Leaotum*) by a barrier of wooden stakes, which encompasses the whole province, and about which guards continually patrol to hinder the *Chinese* from going out, and looking after this root; yet how vigilant forever they are, the *Chinese* lurk about privately in these deserts sometimes to the number of 2 or 3000, at the hazard of losing their liberty, and all the fruit of their labour, if they are taken either going out or coming into the province: The emperor, having a mind that the *Tartars* should have the advantage that is to be made of this plant, rather than the *Chinese*, gave orders in 1709 to 10,000 *Tartars* to go and gather all they could of the *Gin-seng*, upon condition, that each person should give his majesty two ounces of the best, and that the rest should be paid for according to its weight in fine silver; it was computed, that by this means the emperor would get about 20,000 *Chinese* pounds of it, which would not cost him above a fourth of the value: This army of herbalists observed the following order; after they had divided a certain tract of land among their several companies, each company, to the number of 100, spreads itself out in a strait line to a certain fixed place, every 10 of them keeping at a distance from the rest; then they searched carefully for the plant, going on leisurely in the same order; and in this manner in a certain number of days they run over the whole space of ground allotted them; when the time is expired, the mandarins, who are encamped with their tents in such places as are proper for the subsistence of their horses, send to view each troop to give them fresh orders, and to inform themselves if their numbers be compleat; if any one of them be wanting, as it often happens, either by wandering out of the way, or being devour'd by wild beasts, they look for him a day or two, and then return again to their labour as before: These poor people suffer a great

deal in this expedition; they carry with them neither tents nor beds, every one being sufficiently loaded with his provisions, which are only millet parched in an oven, upon which he must subsist all the time of his journey; so that they are obliged to sleep under trees, with only their branches and barks, if they can find them, for their covering: Their mandarins send them from time to time some pieces of beef, or such game as they happen to take, which they eat very greedily, and almost raw; in this manner these 10,000 men passed six months of the year; yet notwithstanding their fatigues, continued lusty, and seemed to be good soldiers.

Plate II. Fig. 5. A shews the root of the plant, which when washed, was white and a little rugged, and uneven, as the roots of other plants generally are; B C D represent the length, and thickness of the stalk, which is smooth, and pretty round, of a deep red colour, excepting near its beginning at B, where it is whiter, by reason of its nearness to the ground; D is a sort of knot or joint, formed by the shooting out of four branches, which all rise from the same center, and divide from each other at equal distances, and at the same height from the ground; the underside of the branch is green, mixed with white; the upper part is much like the stalk, of a deep red, inclining to the colour of a mulberry; these two colours gradually decrease, and unite together, on the sides in a natural mixture; each branch has five leaves, as represented in the figure; it is remarkable, that these branches separate from each other at equal distances, as well in respect of themselves, as of the horizon, and with their leaves form a circular figure, nearly parallel to the surface of the ground; tho' the design of but half of one of the leaves at B be finished, yet the rest may be easily conceived, and perfected in the same manner: P. *Jarroux* never observed leaves so large as these, that were so thin, and fine; their fibres are very distinguishable; and on the upper side they have small whitish hairs; the skin between the fibres rises a little in the middle above the level of the fibres; the colour of the leaf is a dark green above, and a shining whitish green underneath; all the leaves are serrated, or very finely indented on the edges; from D the center of the branches there rises a second stalk DE, which is very strait and smooth, and whitish from top to bottom, bearing a bunch of round fruit of a beautiful red colour; this bunch consisted of 24 berries, two of which are represented by 9, 9. The red skin that covers the berry is very thin, and smooth; it contains within

within it a white softish pulp; as these berries were double (for, they are sometimes found single) each of them had two rough stones, separated from each other, of the size and shape of common lentils, excepting that the stones have not a thin edge like lentils, but are almost every where of an equal thickness; each berry was supported by a smooth, even, and very fine sprig, of the colour of those of small red cherries; all these sprigs rose from the same center, and spreading exactly like the *Radius* of a sphere, make the bunch of berries, that they bear, of a circular form; this fruit is not good to eat; the stone resembles the stones of other common fruit; it is hard, and incloses a kernel; it is always placed upon the same plane, or level with the sprig that bears the berry; whence it is, that the berry is not round, but a little flat on each side; if it be double, there is a kind of depression, or hollow place in the middle, where the two parts unite; it has also a small beard at top, diametrically opposite to the sprig whereon it hangs; when the berry is dry, there remains only a shrivelled skin, that sticks close to the stones, and is then of a dark red, or almost black colour; this plant dies away, and springs again every year; the number of its years may be known by the number of stalks it has shot forth, of which there always remains some mark, as is to be seen in the Fig. by *bbb*, &c. whence it appears that the root *A* was seven years old, and the root *H* 15. in Fig. 6. The flower of this plant according to some is white, and very small; others assured *P. Tartous*, that this plant has none, and that no body ever saw it; but he rather believes, that it is so small, and so little remarkable, that they never took notice of it; and what confirms him in this opinion is, that those who look for the *Gin-seng*, regarding only the root, commonly neglect, and throw away all the rest of the plant, as of no use: There are some plants, which besides the bunch of berries already prescribed, have also one, or two berries like the former, placed an inch, or an inch and a half below the bunch; and when this happens, they say, if any one takes notice of the point of the compass that these berries direct to, he cannot fail of finding the plant at some paces distant that way or thereabouts; the colour of the berries, when the plant has any, distinguishes it from all others, and makes it remarkable at first sight; but it happens sometimes, that it bears none, tho' the root be very old, as that represented in the figure by *H* had no fruit, tho' it was in its 15th year: The *Tartars* sowing the seed in vain without its producing any plant, might probably give occasion to this story, which is current among them; *vis.* that

that a bird eats it as soon as it is in the earth, and not being able to digest it, it is purified in its stomach, and afterwards springs up in the place where it is left by the bird with its dung; *F. Jartoux* rather believes, that the stone remains a long time in the ground before it shoots out any root; and this opinion seems the more probable, because there are found some roots, which are no longer and no bigger than one's little finger, tho' they have shot forth successively at least 10 stalks in as many different years: Tho' the plant above described had four branches, yet there are some that have but two, others but three, and some that have five or seven, which last are the most beautiful; yet each branch always has five leaves, as well as that above represented, unless the number have been diminished by any accident; the height of the plants is proportionable to their bigness, and the number of their branches; those that bear no fruit are commonly small, and very low: The larger and more uniform the root is, and the fewer small strings, or fibres it has, it is always the better; on which account that represented by H is preferable to the other; *F. Jartoux* does not know for what reason the *Chinese* call it *Gin-seng*, which signifies the representation, or form of *Man*; nor could he, nor others who had enquired into it on purpose, ever find it had any resemblance to the signification of its name; tho' amongst other roots there may now and then be found some, which by accident have very odd figures; the *Tartars* with more reason call it *Orbota*, which signifies the chief of plants.

This plant does not grow in *China*, as *F. Martini* affirms from the authority of some *Chinese* books, which make it to grow on the mountains of *Tong-pinsou* in the province of *Peking*; they might be easily led into this mistake, because that is the place where it first arrives, when it is brought from *Tartary* into *China*: Those that gather this plant preserve only the root, and bury together, in some certain place in the earth, all that they can get of it in 10, or 15 days time; they take care to wash it well, and cleanse it with a brush from all extraneous matter; afterwards they dip it into scalding water, and prepare it in the fume of a sort of yellow millet, which communicates to it part of its colour; the millet is put into a vessel with a little water, and boils over a gentle fire; the roots are laid upon small transverse pieces of wood over the vessel, and are thus prepared, being covered with a linen cloth, or some other vessel placed over them; they may also be dried in the sun, or by the fire; but then tho' they retain their virtue well enough, yet they have not that yellow

low colour, which the *Chinese* so much admire; when the roots are dried, they must be kept close in some very dry place, otherwise they are in danger of spoiling, or being eaten by worms.

An Account of the Chalybeat or Spaw Waters; by Dr. Frederic Slare, Phil. Trans. N° 337. p. 247.

Germany abounds much in mineral waters of the chalybeat or iron species; and the *Germans* give them one general name, and call them *Saur Brunnen*, that is *sour wells*, or *springs of water*: The *German* authors call them *acidulae*. ex. gr. *Spadenses*, *Swalbacenses*, vel *Pyromontane*, &c. *Henricus ab Heers* agrees with *Vitruvius*, *Fallopianus*, and the sagacious *Helmont*, and other famous men in justifying the acidity of the several sorts of spaw, but not satisfied with their reasons he assigns others; and after a tedious and obscure harangue, concludes, that they owe their original and great virtues to vitriol, and sulphur; he observes, that vitriol, and sulphur are found in the glebe, or earth, from whence these waters spring; yet he does not give us one proof, or experiment of his having found any vitriol, or true sulphur, or any acidity in these waters: *Dr. Fordis*, a fellow of the *Royal Society*, having examined those waters of the *Saur brun* or *acidula* at *Swaldbac* (so much celebrated for its virtues, and concourse of persons of the greatest quality) gave *Dr. Slare* an account of some ocre, or ferruginous parts, which he had calcined; but in all his experiments it did not appear to *Dr. Slare*, that the water contained one drop of acid by distillation, &c.

That which gave *Dr. Slare* the first suspicion, that the chalybeat waters did not contain any rough, vitriolic, or acid salts in them, was his using a strong iron water, in which he dissolved soap; which he found it lather, and wash his hands well, and afterwards he used a wash-ball, and shaved with it; and he tried several other waters of this sort, which did the same, and much better than any pump water.

1. *Dr. Slare* consulted his palate, and tried whether he could discover any sharpness, or acidity in our *English* chalybeat waters at *Tunbridge*, at *Black-boy*, in the parish of *Franfield* in *Sussex*, *Hampstead*, *Sunning hill* in *Berkshire*, &c. But he was so far from discovering any such thing, that these waters seemed rather to leave behind them a sweetish flavour; thus several alkalious salts of the fixed kinds, if nicely examined, affected his palate. 2. He made experiments with several sorts of such spirits, as are apt to ferment with acids; such as spirit of *Hartsborn*,

born, of *Sal armoniac*, &c. but these caused no fermentation, nor any motion, nor change in these waters. 3. He considered the diseases in human bodies, for which physicians prescribe these waters; and that they were often such as proceeded from sharp, acid, or acrimonious causes, as *Cardialgie*, or heart-burnings, sour vomitings, corrosive *Diarrhoea's*, colics from scurvy and stranguries; and that sweetning and alkalifate medicines are made use of for these distempers. 4. He considers these waters, as containing in them the properties of iron; and he finds by experience, that it is most opposite to acids, being one of their great correctors, and therefore, rather to be reckoned an *Alkali*. 1°. Take some filings of iron, about a drachm weight, and pour thereon about an ounce of the milder acids, such as vinegar, verjuice, or the juice of lemons, and it will destroy the sharpness of these juices; or if you pour on these filings mineral acids, as the very corrosive spirit of nitre, or of salt, or what is called oil of vitriol, they will immediately lose their acidity, be disarmed of their sharp points, and by evaporation yield a salt that will taste sweetish, called by chemists *saccharum Martis*, if duly prepared; which is safely given inwardly, and is esteemed a good alterative medicine. 2°. Steel beaten to a fine powder is, without any farther preparation, given inwardly with great success for stomachic diseases, as in the green sickness, hypochondriac, and various other acid, and acrimonious indispositions. 5. He considered milk to be a very proper and obvious subject to bring this controversy to a plain and unquestionable decision; he made this experiment with all possible exactness; he first tried the chalybeat waters, more particularly the spaw waters, to see whether they tinged with galls; these being very good, he put part of the waters to cold milk; some he made only luke-warm, and some he boiled together, in equal proportions; but they were so far from affording any curd, or coagulation, that they continued several days without becoming sour: The *German* physicians, supposing these waters to be sour, prohibit the use of all *Lacticia*, as if they were as noxious as deadly poisons, whilst any is in a course of their medicinal waters: Dr. *Staro* frequently advised in some cases milk to be given in the evening, thro' a whole course of chalybeat waters, and that with good success; nay he affirms, that some others could not bear the waters without having a third part of milk or more, mixed with them, and have continued them so for several weeks with much benefit; nor does he find the least reason to forbid the use of milk in a course of *Bath* waters, having for upwards of

of a year and half made the best enquiry he could into the properties, virtues, and vices (if any) of these waters. 6. Seeing experiments shew, that those things which are of a sweetening alkalifate nature, do so very well agree with these mineral waters, it will appear by the following experiment that acids do very much disagree. 1°. He put but one drop of oil of vitriol to a large glass full of strong spaw waters, which before the addition of this acid gave a deep purple to the solution of galls; but now would not give the least tincture, tho' he put in four times as much of the galls; from hence he concludes, that the virtues of the chalybeat ingredients, which the Dr. takes to be the life and soul, as it were, of these waters, were so far bound up, or destroyed, as to have lost their cordial, or corroborating faculty; and that the bile, or gall in the human bowels could not be able to separate the chalybeat (which are the only medical) particles, and mix them with the chyle, in order to answer any end in physic: This should be a caution to such as design to make these waters pass better by urine, not to use any acids; it being a common practice to use spirit of vitriol, *Spiritus nitri dulcis*, &c. as a diuretic; unless it should so happen, that they have a design to take off, and divest them of their warm, cordial, or alterative power, and so bring them near to common water; which we are obliged to do, especially in the use of Bath waters in some hot inflammatory cases. 2°. The Dr. concludes with an experiment in favour of our *Alkali's*, viz. that if you put any alkalious salt, volatile, or fixed, such as volatile salt of hartshorn, or of *Sal armoniac*, or fixed salt of tartar, of wormwood, or any other true *Alkali*, you will then destroy the above-mentioned acid spirit, recover the virtue of the waters, and dispose them to give their tincture, as they used to do in their natural state.

An Account of several Urns and Sepulchral Monuments found in Ireland, by Mr. Nevill. Phil. Trans. N° 337. p. 252.

WITHIN a mile of *Castle-doe*, an urn was found in a little island, surrounded with bogs; this island was very dry, light, sandy ground, situated on an isthmus, about half a mile over, between the bay of *Dunsannagban*, and *Lough Kinnevier*; in taking up a flat stone there was found a cavity underneath, which may be call'd a sepulchre, or tomb; wherein the said urn was found, which the person who discover'd it broke, and there was nothing in it but bones, and ashes; in the same tomb there were some bones of one about 10 or 12 years of age; the

tomb stood east and west, the urn was found in the west end; it was the smallest urn Mr. *Nevill* had ever seen, but the cavity wherein it lay was near five foot long, two foot and a half broad, and about the same depth; it was made up of six coarse flag-stones, *viz.* one on each side, one at the head, another at the foot, one above, and one below; the bones were much wasted, and there were but few of them; whilst Mr. *Nevill* staid there they opened three more, which were much larger than the former; one of the three was near the center of the island, and the largest of all; but all were alike made, there was no urn in any of them, and but bones in one, which was the largest; the bones seemed to be those of a man of an ordinary stature; if there had been any in the other two, they were consumed: This seems to have been a common burying place, there being so many of that kind of tombs in it; and one may gather from thence, that at that time they burnt some, and others they did not; because there was an urn found with burnt bones, and other bones unburnt.

There were three such urns found in three small stone chests under a great *Karn*, or heap of stones, near *Banbridge* in the county of *Downe*; the like urns were found near *Omagh* in the county of *Tyrone*, in the like chests under two heaps of stones; there was an urn found in a little sandy hill near *Cookston*, on the road to *Liffon* in the county of *Tyrone*; it was cover'd with a large rough lime-stone, which being remov'd, the urn was discover'd in a hole, encompass'd with six stones of equal bigness, which form'd a hexagon, wherein the urn stood; the water that had fallen on the urn, from the lime-stone, or the air condensing, had petrified, and made a stony crust on the outside thereof; there were some bones, and ashes found in it: At *Dungannon* in the same county, a servant of Mr. *Nevill's* working in a sand-pit near the town, struck on an urn, the largest Mr. *Nevill* ever saw; it was found with the mouth downwards, the bones and ashes on a flat stone, and the urn covering them; it would have contained about three quarts, and had been better burnt in the fire than they commonly are; it had no stones about it as the others had, but was buried in the earth about a foot under ground; as they dug the bank for sand, the place where the dead body was burnt was discover'd by the coals, and pieces of bones, which were spread a great way, about a foot under ground: Near the same town there is a place call'd *Killimeillo*, *i. e.* lousy cell, or lousy burying-ground, where on the top of the hill, are two circles of dry stone about 10 yards in diameter each;

each; they meet on two sides, and form the figure of 8, as it were; Mr. *Nevill* supposes, when first form'd, they made a dry wall for two distinct burying-places, one for the men, the other for the women; or rather two repositories for urns; one *James Hamilton* upon digging and entering within one of the circles found three urns in three several holes, set round with six stones, and cover'd over with flat ones, and other stones thrown on the top: On the same hill about 30 yards distance to the eastward of these circles was found the altar, whereon they us'd to burn their dead, overgrown with earth, and green sod; this altar was made of dry stone, eight foot long and four foot broad, the coals and bones were fresh among the stones, and the stones were burnt with the fire; at the east end of this altar there was a pit, which was likewise overgrown with earth and green sod; upon opening which it was found to be the receiver, where they swept in all that remained on the altar after burning; upon digging deep, the substance was all alike, black, and greasy; it had ringed the hill in a streight line from the pit to the bottom thereof; Mr. *Nevill* adds one more he saw, besides several that are found yearly, to shew that this was the method the *Pagan Irish* used to bury their dead in, tho' the people know nothing of it, either by history, or tradition; in the county of *Farnagh* upon a hill, over *Wattle-bridge*, there has been a large heap of stones, the lower part of which is surrounded with very large ones standing on end; under this heap were found some urns in stone coffins; these he supposes were the urns of some persons of distinction; the heap was very large, and the stones round it huge, and very numerous, so that it must have cost a great deal of pains to have brought them thither; or, perhaps there might have been a battle there, and some of the chief officers who were kill'd, might have their bones interr'd in this place, and the army have made that great heap over them: Mr. *Nevill* had seen several such heaps in *Ireland*, and he doubts not but that they are all monuments for the dead.

An Account of a great Number of Urns dug up at North Elmham in Norfolk; by Mr. Le Neve. Phil. Trans. N° 337. P. 257.

IN the parish of *Elmham*, upwards of half a mile from the town, there is a close call'd the *Broom-close*, lying on the west side of the road from *Elmham* to *Beetly*; some labourers repairing the fence on the south side of the close, in the bottom of the ditch accidentally pitch'd upon an urn; and digging on, they

they found two or three more; upon a farther search, and upon digging first under the hedge, and afterwards farther into the close, there were found large quantities of them, and several very near each other; in the compass of about a rood of land, *i. e.* a quarter of an acre, there were dug up near 120 urns; the close where they are found is high land, and this place the highest part of it; the soil is a sharp gravel, and very dry, and lies next to a highway; as to the urns they are generally of the same shape, but of very different sizes; and their shape conformable to the representations usually exhibited in the descriptions of urns, *viz.* the bottom narrow, a little flattened, (and in some quite round) wider upwards, and the top contracted to a narrow mouth; the earth coarse; the work rough, and uneven, but generally well burnt; some of them slightly wrought, and indented (expressing very little skill or care) and others plain: The size is various; some containing a quart, some two, some three quarts, and one unopened, which he supposed to contain a gallon: The pots are very tender, when they first come out of the ground, and frequently suffer by the wounds of the spade; most of them are broken more or less in taking them up, and hardly any that have not their mouths broken; of which several seemed to be done, as they lay in the ground, by the weight of the earth pressing upon them, or the feet of horses going over them, as appears by the broken pieces found a good way down among the earth: The urns are found at uncertain depths; some very near the surface, some two, some three spits deep, which is the deepest has been dug hitherto: The contents were generally the same; Mr. *Le Neve* opened several of them, and found in all of them a great many pieces of broken bones, some black with burning, others turned to ashes, with some bits of coarse glass run, and sticking to the bones; which whether it proceeded from any thing of that kind burnt with the body, or only the sandy earth vitrified with the strength of the fire, as he is apt to think, is uncertain; besides, he found some pieces of brass, that were run, others much burnt, and some untouch'd, with some pieces of iron, but effaced with rust in such a manner, that it was hard to determine their figure or use: Mr. *Le Neve* had some knives, and other odd things, but much corroded with rust; whilst the brass, which is not burnt to pieces, remain'd generally firm, and entire; there is one thing remarkable, *viz.* there was found a great many small nippers (such as hairs are pulled out with) commonly of brass; and most of them so entire, that the edges were full square, and the spring as strong as ever: As for coins,

coins; none were met with; he heard of two in the possession of a certain person of *Elmhurst*; and he himself had one found in an urn, but very imperfect; what remained of the impression, look'd more like *British* than *Roman*; but by the shape of the coin, and by the metal it should be the latter, tho' it was not easy to determine: As it is pretty certain, that the urns are *Roman*, and consequently, that the quantity found of them must denote a station, or colony of that people; so he leaves others to determine the place, where that station was appointed, upon what occasion, and at what time it might be planted there.

Observations on Lough-Neigh in Ireland; by Mr. Nevill.

Phil. Trans. N^o 337. P. 260.

AS to *Lough-Neigh*; so much talk'd of for its changing wood into stone, Mr. *Nevill* affirms, that there is no such petrifying quality in that water; for, he saw several trees lying in the verge of the lough, some of which he believ'd had lain there some hundreds of years, which had been overturned by the lough's incroaching on the land, where large woods had grown; and several roots of huge trees were standing in their proper places, where the water had prevail'd on the land; and no alteration in the wood at all; but it was firm and sound, without any petrification: One Mr. *Brownlow* told Mr. *Nevill*, that he had ordered holly stakes to be driven into the ground within the verge of the lough, some of which continued there several years, but that he found no alteration: Yet notwithstanding all this, there have been large quantities of such sort of stone, resembling wood, found upon the strand after great floods, and storms of wind, that put the lough into a commotion; the waves breaking down the banks, incroaching on the land, and throwing down trees, by which means this sort of stones are discover'd; and if ever they were wood, they were petrified by the earth, and not by the water; of which kind Mr. *Nevill* saw several pieces, both large and small, some resembling oak, some ash, and others holly with bark, grain and knots like wood: so that any would be apt to judge by the naked eye, that it was wood, till they came to try it: He had a piece about 16 inches long, that look'd as if it had been a large chip cut out of the side of an oak-block, with the bark on; and in cutting such large chips, there generally happen some flaws, so that the parts are separated at one extremity, whilst they remain firm at the other, which was the case in this chip; he could have rais'd several such splinters of this chip, some larger, and some smaller; and when thus rais'd,

said, they shipp'd down like a spring; some of these stones appear'd at one extremity, as if rotten and decay'd wood; but upon trying it, he found it as much stone, as any other part thereof.

Lough-Neagh is computed to be 24 miles long, and 12 miles broad, and navigable from *Charlemount* to *Portlough*, which is about 35 miles; it does not abound with many sorts of fish, but those it has are very good, such as salmon, trout, pike, bream, roach, eels, and pollans, term'd by the *English* fresh water herrings, with which last it abounds; they catch them in the summer with sieves, as they do herrings; in shape and bigness they resemble very much the largest smelts, being full of very large bright scales, and they eat well, when fresh; these were supposed to be a fish peculiar to that lough; but *Lough-Earne* has the same sort, but not in such great plenty; they are generally caught here in their eel-nets, when running to the sea; so that Mr. *Nevill* takes them to be that sort of fish call'd shads, and that the large ones come from the sea, as the salmon do, and leave their spawn in the lough, which, when they grow large, go to the sea, and there come to their full growth; and this is confirmed by the salmon fishing at *Colratna*, where they catch several of the large ones going up to the lough, there is one sort of trout in *Lough-Neagh* very large; Mr. *Nevill* saw one that weigh'd 30 pounds; and the largest salmon he ever saw, weigh'd no more than 33; this sort of trout the *Irish* call a *Budagh*.

That there is some healing quality in the water of this lough is certain; but whether diffus'd thro' all its parts, is not known; there is a certain bay in it, call'd the fishing-bay, about half a mile broad; it is bounded by the school-lands of *Dungannon*, hath a fine sandy bottom, and not a pebble in it, so that one may walk with safety and ease, from the ankle to his chin, upon an easy declivity, at least 300 yards before a man shall come to that depth; Mr. *Nevill* himself has been there several times, and he always observ'd, that as he walk'd, the bottom changed from cold to warm, and from warm to cold, and this in different spots thro' the bay; several others have made the same observation: The first occasion of taking notice of this bay for cure, was in the reign of King *Charles II.* viz. One Mr. *Cunningham's* son had the *Evil* to such a degree, that it run upon him in eight or ten places; he had been touch'd by the King, and all imaginable means us'd for his recovery, but all to no purpose; when all hopes of his recovery

were

were past, he was carried to the lough, where he was bath'd; and in eight days time, bathing each day, all the sores were dried up, and he was cur'd, and grew very healthy, married, got children, and liv'd nine or ten years after: Upon so remarkable a cure, several came there, who had running sores, and were cur'd in a little time after; and sick cattle are likewise brought thither, and driven into the water for their cure; and people believe they receive benefit by it; Mr. Nevill knows, that it dries up running sores, and cures the rheumatism by frequent bathing; and drinking the water he was told will stop the flux; and he thinks it one of the pleasantest bathing places he ever saw.

An Account of a Woman that had lain six Days cover'd with Snow, without receiving any Nourishment, &c. by Mr. Bowdich. Phil. Trans. N^o 337. p. 263.

ONE Joanna Crippen of Chardstock in Dorset, a spinster of yarn or worsted; going on the 24th. of January, being Chard market, to her masters for work, and coming home with some of her neighbours, it happened to snow very hard, and very deep, so that she was forced into a cottage for shelter; but as they would not let her lodge there that night, she was obliged to proceed on her way home; but she had not gone a quarter of a mile, before she was obliged to lie down under a hedge; in which place she lay from Monday evening about six o'clock, till Sunday following about four in the afternoon; and then she was discovered by several of the neighbours, who went out with poles, shovels, &c. to search for her, and at length found her buried in four foot deep of snow, or thereabouts; one of the men thrusting at her with his pole, found she was there, and alive, and upon opening the snow, she immediately spoke, and beg'd he would not poot her, as she termed it, too hard; for, she was almost naked; and some women coming to take her forth, they found her without stockings, or shoes, an old whittle about her shoulders, with a large hole in it, which she had eat thro'; the snow melting down upon her she drank to quench her thirst; she had a mortification on one of her great toes; was very sensible at the first taking out, and still continued so; and she knew every body perfectly well; and yet she had taken no manner of food all the time of her being in the snow.

An Account of the subsidings of part of a Hill near Clogher in Ireland; by the Bishop of Clogher. Phil. Trans. N^o 337. P. 267.

LET ST Plate II. Fig. 7. represent part of the ridge of a hill, gradually rising from S to F for near half a mile; and STWU the north side of the hill, with a declivity from S to U and from T to W; the perpendicular height at X to the plane of the bottom at Y 130 feet, and the slop'd line or hypotenuse XY 630 feet, the declivity pretty uniform from X to L, and from L to Y considerably steeper, the bank AEFD overgrown with shrubby wood; all the ground on the side of the hill being firm, green and arable, of a mix'd soil, clay and gravel, but more clayey: On *Tuesday* the 10th of *March* 1712-13. in the morning the people observ'd a crack or rent in the ground like a furrow made by a plough, going round from A by BC to D; they imputed this to what they call a thunder bolt, because there had been thunder and lightning on *Monday* night; but on *Tuesday* evening a hideous dull noise rais'd their curiosity; and they observ'd, that the whole space ABCD, containing about three *Irish* (i. e. 4 and $\frac{1}{2}$ *English*) acres had been all day in a gentle motion; and the noise continued all night, occasion'd by the rubbing of bushes, tearing of roots, rending and tumbling of the earth; the motion ceas'd on *Wednesday* afternoon, when they observ'd, that the bushes on the bank EF were remov'd, some standing, and others overthrown, to the plain meadow Yy; the green ground above EF, when it come to the top of the steep part at EF rent with hideous chasms, 10, 15, or 20 foot-deep, and tumbled down in rolls, of a yard or two thick, and 10 or 20 yards long, and broad; not unlike a smooth water breaking over a cataract, and tumbling in waves below; there was a precipice at the top Xx 65 feet perpendicular height, forming the sloping line Xx 126 feet; the ground from x to L, was made more level, the whole perpendicular height of x not exceeding the plane of L above 30 feet; but the ground at L in the whole line from E to F was mounted above 20 foot higher than the unmov'd ground on either side at E and F; and the height of L above the plane of y is 55 feet: There was a ditch HI went cross the ground, which being broken off at o o was remov'd, together with the moving part, 34 feet lower down than the unmoveable part; but at the bottom y it was tumbled 60 feet over the plain meadow; the breadth at the bottom

bottom *ab* was 400 feet, and at *cd* about 300: The whole face of the precipice *Xx* is of a blue clay, mix'd with several little blue stones; the metal, when dry, is very hard, but upon any rain softens to a kind of mortar, without the degree of roughness and stiffness that is natural to clays; it is very like that gravel or sand (as they call it) which is somewhat of a grey marly nature, and with which they so much improve the plow'd land in *Ireland*: About *x* there are chasms, or gappings full of water, which make a rill down the *Hill* *B E A*, but in no greater quantity than might have been expected from a well dug to a less depth; tho' the bishop was told, that there were holes in the higher mountains, that receiv'd water under ground, yet he could find no such thing, nor any symptoms of a current under ground, either where it enters or rises, in all the neighbouring ground for some miles; it seemed to him that there had been no vacancy under ground to receive the subsiding earth; for, what the bank *E L F* is rais'd higher, and what is tumbled down to the plain *ab* may very well compensate the subsiding at the precipice *Xx*.

It is to be observ'd, that before the rupture, the declivity from *X* to *L* was not altogether uniform, but was hollower where *x* is now than the neighbouring parts; it might have been, by the description he had from the people, 10 foot deep in the middle, and 100 foot diameter; and they have a tradition, that this was occasioned by a subsiding before the wars in 1641, the oldest *Epocha* the native *Irish* know: It lies in the lands of *Stat-beg*, two *English* miles south west of *Clogher*: The Bishop diligently enquired of the neighbours, if they felt any shocks, or indications of an earthquake, but he did not find the least appearance of any: They imputed it to the great and constant rains they had had the preceeding harvest and winter, which had soaked, and steeped all the ground; but he cannot conjecture after what manner they should produce this effect; for, it is impossible any water should stand on the ground, or in the neighbouring parts, it being all on the declivity of the hill.

An Account of some Ancient Trumpets, and other Pieces of Antiquity, found in the County of Tyrone in Ireland; by Mr. Nevill. Phil. Trans. N^o 337. P. 250.

THERE were eight trumpets found together in the lower barony of *Dungannon* in the county of *Tyrone*; four of them were of one make, and four of another, they were of cast brass, of the thickness of an *English* half crown.

Plate II. Fig. 8. represents a trumpet 24 inches long, according to the turn, or arch it forms, and it is three inches in diameter at the large extremity, and solid for about two inches at the small extremity, with a loop at top to hang it by, and another loop between the solid part, and the mouth-piece; the mouth-piece is oval, five inches from the solid extremity, an inch and $\frac{1}{2}$ long, and an inch wide; its sides are smooth, round, and even, easy for a man's lips, but it will not yield any sound by blast or blowing, as a horn does, but by tooting it will; one may raise his voice in it to the highest pitch, and bring it to the lowest bass.

Fig. 9. is a trumpet 26 inches long, an inch wide at the small extremity, and three inches at the other; but seems to be imperfect for want of a mouth-piece; the small end seems to be fitted to receive one; on the back at *a* there is a hole, and another under the moulding at *b*; the first Mr. *Nevill* supposes was for fastening the mouth-piece, the second for stopping, or opening with the finger to alter the sound at pleasure; but as it is now, it cannot be sounded; for, it is noways fitted for the mouth, it being thin, and sharp, as appears by the figure.

Mr. *Nevill* could not find in any *Irish* story, or by tradition, any account of this sort of trumpet, nor indeed, of any other; from whence he gathers they are of great antiquity; for, had they been of use at, or since the first of the *English* conquests, there would have been some hints of them; and, therefore, Mr. *Nevill* concludes they were used when the country was *Pagan*; not in military affairs, but by their priests at their funeral rites, when they buried their dead, and bore a part with those who cried at those funerals, or made an howling sort of a noise, called by the natives *corrannech*, which is used among them to this day.

Fig. 10. represents an instrument of fine gold; but what to call it, or what its use Mr. *Nevill* could not determine; there were five of them found in different places, three of which Mr. *Nevill* saw; one was found near *Coof-hill* in cleansing a ditch, under the side of a large stone, that was one of three placed in the
furne

form of a triangle; whether set in this form, as a mark to find this thing when hid, or whether for any other use, he could not conjecture; but he has seen stones in several parts of *Ireland* set in this order; there were some other pieces of gold found with this; one he was informed, somewhat resembled a scepter, about 18 inches long, and another was round like a large medal, as thick as two crown pieces, and as broad as the palm of a man's hand, with odd characters on it; the beauty and colour of the piece of gold represented by the figure surpassed any Mr. *Nevill* ever saw; it had been cast, and not wrought with the hammer; the two cones were two inches in diameter each, and two inches asunder from each other; the bow, or handle was made like an arch, about $\frac{1}{2}$ of an inch thick; the handle was round, tapering towards both extremities, where it was fixed to both the cones; the goldsmith that procured it told Mr. *Nevill*, that when he had wrought part of it, he could not bring the gold to the right colour again, tho' it was in fineness equal to standard; this Mr. *Nevill* thinks, could be no ornament for the body; perhaps it might be for some religious uses; but whether *Papish*, or *Pagan* he leaves others to determine.

An Account of a Person who had a new Set of Teeth after 80 Years of Age, with Observations on the Virtues and Properties of Sugar; by Dr. Slare. Phil. Trans. N^o 337. P. 273.

DR. *Slare's* grand-father, of an old *English* family in *Bedfordshire*, made it his daily practice to lick, or take up as much sugar, as his butter spread upon bread would receive, for his constant breakfast, unless sometimes he happened to exchange it for honey; he frequently sweetned his ale and beer with sugar; he had sugar put to all the sauces he used with his meat; at 80 years of age he had all his teeth in his mouth, strong and firm; he never had any pain, or soreness in his gums, or teeth, and never refused the hardest crust; in his 82 year, one of his teeth dropped out, and soon after a second, which was one of the fore-teeth; he bid the Dr. feel the cavity, where he struck his nail upon a bone: In short, all his teeth dropped out in two or three years, and young ones filled up their room, and he had a new set quite round; his hair, from a very white colour, became much darker; he continued in good health, and strength, without any disease, and died in the 99 or 100 year of his age, of a *Pleurisy*, as the Dr. conjectures, for want of bleeding.

This made the Dr. vindicate sugar, and shew the unjust character given of sugar by the famous Dr. Willis, who charges it with a corrosive quality, as bad as *Aqua fortis*, which he calls *Aqua stygia*; Dr. Stare examined it, and found the charge unjust, and that sugar contained no more pernicious substance in it, than milk, honey, *Manna*, nay even bread itself.

Fig. 11. represents the form of the crystals, or salts of sugar, with two bases opposite, equal, and parallel, the other are parallelograms.

Fig. 12. represents the basis of the preceeding Fig.

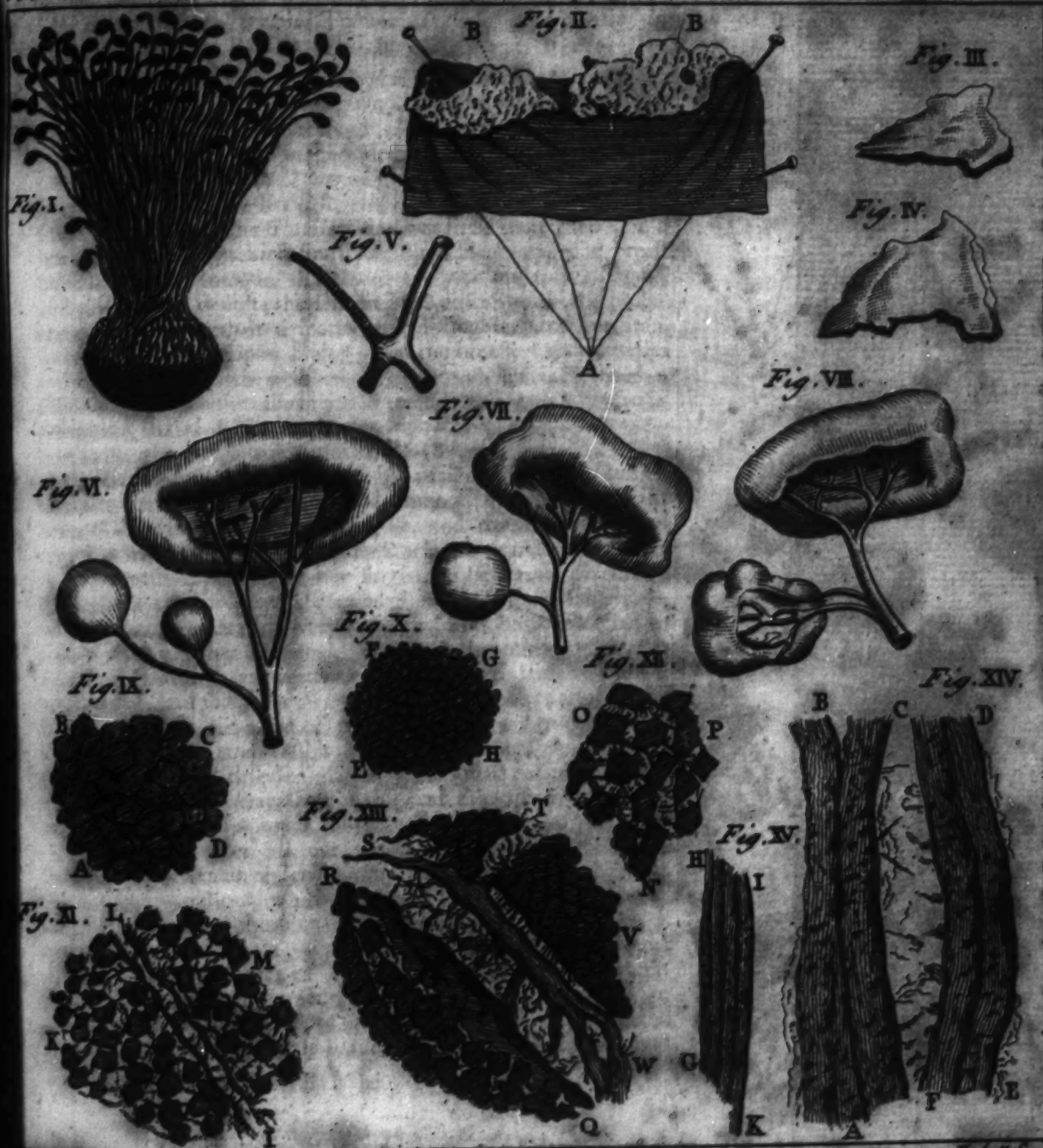
The difficulty of shewing the figure, that sugar naturally shoots into, lies in this, to wit, that all other salts shoot, or crystallize, and make their figure in a cool place; whereas, sugar will crystallize only in a hot stove, and is more apt to be compounded, and not to shew its true primitive texture; thus it happens to snow, which in its true simple shape is an hexagon; but it cannot always be discovered single; this is still more easy to be accounted for than snow; and the Dr. has been able to pitch upon such parcels of sugar candy, as represented by Fig. 11. and which is a prism. The Dr. never questioned, but that it was a true salt, having all the properties of one.

Remarks on an undescribed Plant, and other Observations in Wales; by Mr. Ehwyd. Phil. Transl. N^o 337. p. 275.

PLATE III. Fig. 1. represents a remarkable sea plant, met with in dredging for oysters near *Lhan Danwg* in *Meirionnyddshire*; the whole plant is of a straw colour, and much of the same bigness with the figure. The stems are hollow, and replete with a kind of thick reddish liquor, rather resembling blood, or gore, than the juice of a plant; insomuch, that it seems referable to the *Zoophyts*: Upon pressing these stems at the bottom between your fingers, the red liquor is forced up, and causes the drooping flowers, or seed-vessels to mount erect.

We discovered a sort of marble in that county, which, when polished, represents a number of small oranges cut acrois; the reason of which is a vast number of *Porus* or *Alexoniam* stuck thro' the stone; this might serve very well for inlaying work, as tables, windows, cabinets, closets, &c. and would make curious salt-cellers.

Wales affords a good deal of allum and copperas; particularly, *Pembroke-shire*, and *Caermarthen-shire* for the former, and *Meirionnydd-shire* for copperas, where Mr. *Llwyd* saw a large vein of *Pyrites* strongly impregnated.



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An Account of a scirrhus Tumour, included in a Cystis; by
Mr. Richard Russel. Phil. Trans. N^o 337. p. 1276.

AUGUST the 18. 1713, Mr. Russel was sent for to one Mrs. Smith at Lewes in Sussex, who had been brought very low by a fever; which, from her cough, sharp pain under her breast, and other symptoms was judged pleuretic; but upon a discharge from her breast of a thin Serum or gleet, all symptoms vanished: When Mr. Russel saw her first, the liquor was discharged by a small pin-hole near the Papilla, and was little more than would have wet a handkerchief fourfold; upon examining the breast he found a large tumour, that lay deep, yielding to his fingers, and passing like dough; he searched the abscess with his probe, and twisted out with it a matter resembling saw-dust, or bran, mixed with hair: Upon laying open the breast, he separated a cystic tumour, which weighed eight ounces, and contained a solid matter like the above-mentioned, mixed with a substance resembling hair: Upon enquiring into the manner of this tumour's rise, the patient told him, that 38 years before, she had received a contusion in that breast by a fall from a horse, which was attended with great pain, and fluxion; insomuch, that the veins of her breast appeared varicose, and turgid, as in a cancer; but her pain ceasing, they sunk and left an indolent tumour in her breast, supposed by her surgeon to be a true Scirrhus; since which time it always continued nearly in the same state, without pain; encreasing very little in bulk, but obstructed in such a manner, that she could not nurse her child with that breast: The tunic was pretty thick, nourished with very small vessels, but had formed a Scirrhus of the glands it adhered to, by keeping up a distension of parts, till there was a cohesion of their membranes, and vessels: Mr. Russel doubts not, but that this was a body of diseased glands, which had suffered a colliquation by some extravasated fluid, and that the membrane of the tumour was their proper tunic.

After this manner all our tunicated tumours seem to be formed; for, when an obstruction proceeds to extravasation, there is a liquor poured out, which consists of such particles, that by degrees make a colliquation of the glandulous flesh, which is not very sensible of pain; and by degrees the Capsula becomes distended with a matter of a very different consistence, which gives the name to the tumour, either Srentoma, Atheroma, or Meliceris: Thus pour oil of olives upon spirit of nitre, and your oil first becomes a little hardened, then of the colour, and

consistence of marrow, till by degrees it is hardened into a white fat, resembling that of animals. The possibility of this coagulation, and digestion we may the more easily be induced to believe, if we consider, how often we find the glands of the *Viscera* petrified, without any degree of pain, or the membrane in any measure destroyed; the truth of which, every one, conversant with the dissection of morbid bodies, must have observed.

An Account of a Quarry of Marble discovered in the County of Fermanagh in Ireland; by Mr. Nevill. Phil. Trans. N^o 337. P. 278.

MR. *Nevill* discovered a marble quarry in the mountains; it lies on the north side of *Calcagh*, in the parish of *Kilashur*, and county of *Fermanagh*; there are marble rocks, 50 or 60 feet of perpendicular height, laid bare by subterraneous rivers, which by degrees have washed away the earth, and loose stones; there are several large pits fallen in on the sides of the great mountain; several of them in a small compass of ground, so that it is dangerous travelling near them; there are several caves formed, some very large, whose sides and arches are of marble; some of a liver-colour, variegated with white in several little figures; some of a light blue, variegated with white; but he could find no entire white or black amongst them.

Remarks on the Plague at Copenhagen in 1721; by Mr. Chamberlayne. Phil. Trans. N^o 337. P. 279.

THIS distemper began to shew itself first in this city, about the beginning of *July* 1721; it increased till the beginning of *September*; after which it decreased by little and little to the end of the year, at which time it totally ceased: It appears, that before this distemper there were about 60,000 souls in *Copenhagen*; whence they infer, that there is born every year, about 2000, and that there dies nearly the same number, which multiplied by 30 makes 60,000. In the six months this distemper continued, it is computed it carried off about 25,000 souls; it is true, that the public lists reckon but 22,555; but it is agreed by all, that in the last week of *August*, and the two first weeks of *September*, each carried off upwards of 2300 souls; besides a great many that died, of which no notice was taken: Almost the very same happened two years before at *Dantzick*: where before the plague broke out, there died weekly from 40 to 50; but the number of the dead increased by degrees to the beginning of *September*; so that in the first week

of that month there died 2205 souls; in the second week 2070, and in the third 2073; after which the mortality decreased to the end of the year.

It is observable, 1. That there were some houses which escaped the infection; but that there were few, where it did not carry off more than one or two persons; and that there were several, in which it did not leave a soul alive. 2. That generally speaking, this distemper was most fatal to the meaner sort of people, there scarce dying any person of note; but on the contrary a great number of the poor; which may be attributed to several causes; the first and most general cause is their nasty manner of living; the second is, that this sort of people live very close together, and heaped, as it were upon each other; so that sometimes there are four families in one room; the third is the foolish curiosity they have of seeing the dead bodies; and fourthly, a great many of them are so bigotted to the *Turkish* notion of fate, that they say, if it pleases God I should die of this distemper, I shall not escape it; and if it be his pleasure that I shall live, I cannot die; and upon this notion they go abroad every where, and so catch the infection; there are some of them also, that make no scruple of lying in the same beds, where others have died: The three sorts of trades of which there died most, were coffin-makers (which took measure of the dead bodies) surgeons and shoe-makers: The care that was taken, and the medicines that were used did considerable service; Mr. *Chamberlayne* was told that *The-riaca* did little good; and they also observed the same thing at *Dantzick*.

Some Anatomical Observations; by Mr. *Chefelden*. Phil. Trans. N^o 337. p. 284.

PLATE III. Fig. 1. shews the beginning of the *Aorta*, or great artery, from the heart of a woman who died of a dropy; A the *Aorta*; BB two chalk-stones which possessed the place of the semi-lunar valves: The left ventricle of the heart was dilated to twice its natural magnitude; these stones were supposed to have occasioned the dropy, by obstructing the valves, and hindering a regular distribution of the blood.

Fig. 3. shews a bone extracted from the *Falk*, or first process of the *dura Mater* of a man, who died of violent headaches.

Fig. 4. shews a bone taken from between the ventricles of the heart of a man who died hydropical and tabid; in this subject the whole *Pericardium* adhered to the heart.

Fig.

Fig. 5. shews the optic nerves, the right nerve being wasted and discoloured; both the eyes appeared to be very good; Mr. Cheselden had not an opportunity of enquiring into the case of this person; but he supposes it must have been a *Gutta serena*: He opened another eye of a man, who died of that distemper, in which he found that part of the nerves, which is within the *Cranium*, crushed flat by the anterior lobes of the brain, their ventricles being full of *Lympha*.

Fig. 6. represents three spleens taken from one subject.

Fig. 7. two spleens taken from a man.

Fig. 8. two spleens taken from a woman.

It is to be observed, that in all these three cases of the spleens, each had proper vessels, but the arteries only are there represented; and that the spleens in each subject taken together were but equal in magnitude to the one we usually meet with.

A heart, with the *Vena Azygos* inserted into the right auricle; and the descending *Cava* coming round the basis of the heart, above the *Aorta* and pulmonary vessels, to enter the auricle at the lower part with the ascending *Cava*.

An ureter double two thirds of its length, next the kidneys, and distended by stones passing thro' it.

The *Tuba Fallopiana* impermeable, and without the *Ala Vespertilionis*, the outer extremities being connected to the *Testes*.

An Account of a contagious Distemper, seizing the black Cattle in the Venetian Territories, and especially about Padua; by Dr. Bernard Ramazzini. Phil. Transf. N° 338. p. 46.

ABOUT the year 1710-11, such a violent contagion had seized the black cattle, as like an increasing fire, could neither be extinguished nor stopped by any human means: It first began to be observed a little in *Agro Vincentino*, but soon discovered itself more openly in the country, spreading every way, even to the very suburbs of *Padua*, with a terrible destruction of cows and oxen: It was also taken notice of in several places in *Germany*: On this distemper Dr. *Ramazzini* made a particular dissertation; wherein he enquired into its causes, and shewed what remedies might be used to put a stop to its violent course.

It was sufficiently evident, that this distemper in the cow kind was a true fever, from the following symptoms; as a coldness, rigor, and a standing up of the hair, which was soon succeeded by a violent sharp burning, with a quick pulse; that this fever was malignant, mortal, and pestilential, its concomitant symp-

toms plainly shewed; such as great uneasiness, with a difficulty of breathing, great pantings, with a sort of shorting; and at the beginning a kind of *Stupor*, and drowsiness, a continual flux of a strong scented matter from the nose and mouth, and a very fetid dung, sometimes with blood; all rumination ceased, and pustules broke out all over the body on the fifth or sixth day, resembling the small pox; they generally died about the fifth or seventh day, very few of them escaping: Dr. *Ramazzini* deduces this distemper from a contagious original; he tells us, that out of a great drove, such as the merchants bring yearly into *Italy* out of *Dalmatia* and the bordering countries, one beast happened to straggle from the rest, and to be left behind, which a cow-herd finding, brought to a farm belonging to Count *Boromeo*, canon of *Padua*; this beast infected all the cows and oxen of the place, where it was taken in, with the same distemper it laboured under itself; of which they all died in a few days, excepting one only, that had a rowel put into its neck.

It is no surprising thing, therefore, if from the *Effluvia* (like an atmosphere, proceeding from the sick cattle) of those that were dead, and from the cow-houses, and pastures where they were fed, and by that means infected, and chiefly from the cloaths of the cow-herds themselves, this infection falling upon a proper subject, should diffuse itself so much: When, therefore, this subtle venomous exhalation happens to meet with any of the cow-kind, joining itself with the serous juices and animal spirits, whilst it is carried all over the body, it disorders the natural consistence of the blood, and corrupts the ferments of the *Viscera*; whence it follows, that the natural functions of the *Viscera* are vitiated, and the necessary secretions stopped; for Dr. *Ramazzini* affirms, that this poison is of that kind, which rather fixes, and coagulates, than dissolves the blood; as appeared not only from the above-mentioned symptoms, accompanying the distemper, but that upon opening the dead carcases, whilst still hot, little or no blood run out.

In the dead bodies of all the cattle, it was particularly observ'd that in the *Omasus*, or paunch there was found, a hard, compact body, firmly adhering to its coats, of a large bulk, and an intolerable stench; in other parts, as in the brain, lungs, &c. there were several *Hydatides*, and large bladders filled only with wind, which being opened, emitted a noisome stench; there were also ulcers at the root of the tongue, and bladders filled with a *Serum* on its sides: That hard and compact body resembling chalk, in the *Omasus*, the Dr. takes to be the first effect of the

contagious *Miasma*; the infection was not caught from the air, provided due care was taken in burying the dead bodies.

As for the cure of this distemper; he recommended bleeding, burning on both sides of the neck with a broad, red-hot iron, making holes in the ears with a round iron, and putting the root of hellebore into the hole, a rowel, or seton, in the dew-laps under the chin; he also ordered the tongue and palate to be often washed and rubbed with vinegar, and salt: As for the pharmaceutical part, he recommended alexipharmics, and specific cordials; and from the vegetable kingdom three ounces of jesuit's bark, infused in 10 or 12 pints of cordial water, or small wine, to be given in 4 or 5 doses, which is to be done in the beginning of the fever, when the beast begins to be sick; from the animal kingdom, two drachms of *Sperma Ceti*, dissolved in warm wine; from the mineral kingdom, *Antimonium diaphoreticum*; against breeding of worms, an infusion of quicksilver, or *Petroleum*, and milk to be given; and lastly, as to the food, he recommends drinks made with barley, or wheat flower, or bread, like a ptisan, fresh, sweet hay made in *May*, and macerated in fair-water; in the mean time the cattle to be kept in a warm place, and clothed, that they may be exposed as little as possible to the cold air; fumigations to be daily made with juniper-berries, *Galbanum*, &c. in the cow-houses: As to prevention, the Dr. enjoined care in cleansing the stalls, and scraping the crust from off the walls; as also care to be taken of their food, that it be good; the hay or straw not spoiled by rain in the making, and he judges, that their food ought to be but sparing; he recommended also friction, rubbing and currying, not only with the hand, but with a curry-comb and brush, with setons under the chin, made with a hot iron run thro' the part, and kept open with a rope put thro' it.

Recipe Veronica, Pulmonariae, Hyssopi, scordii ana M. iv. Rad. Aristolochiae rotunda, Gentiana, Angelica, Petasitidis, Tormentillae, Carlinae ana unc. 12. Bac. Lauri & Juniperi ana unc. 12. Misce, fiat pulvis.

Bleed the cow, and give her every morning for three or four mornings, an ounce of this powder in warm beer, with a horn: If the cow's illness continue, after intermitting of two or three days, repeat the medicine for three or four days more.

A new Method for making Logarithms, and vice versa, for finding the Number corresponding to a Number given, by Help of the following Table; by Mr. John Long. Phil. Trans. N° 339. p. 52.

Log.	Nat. Num.	Log.	Nat. Num.
0,9	7.943282347	0,0009	1.002074475
0,8	6.309573445	0,0008	1.001843766
0,7	5.011872336	0,0007	1.001613109
0,6	3.981071706	0,0006	1.001382506
0,5	3.162277660	0,0005	1.001151956
0,4	2.511886432	0,0004	1.000921459
0,3	1.995262315	0,0003	1.000691015
0,2	1.584893193	0,0002	1.000460623
0,1	1.258925412	0,0001	1.000230285
0,09	1.230268771	0,00009	1.000207254
0,08	1.202264435	0,00008	1.000184224
0,07	1.174897555	0,00007	1.000161194
0,06	1.148153621	0,00006	1.000138165
0,05	1.122018454	0,00005	1.000115136
0,04	1.096478196	0,00004	1.000092106
0,03	1.071519305	0,00003	1.000069080
0,02	1.047128548	0,00002	1.000046053
0,01	1.023292992	0,00001	1.000023026
0,009	1.020939484	0,000009	1.000020724
0,008	1.018591388	0,000008	1.000018421
0,007	1.016248694	0,000007	1.000016118
0,006	1.013911386	0,000006	1.000013816
0,005	1.011579454	0,000005	1.000011513
0,004	1.009252886	0,000004	1.000009210
0,003	1.006931669	0,000003	1.000006908
0,002	1.004615794	0,000002	1.000004605
0,001	1.002305238	0,000001	1.000002302

Log.

Nat. Num.

Log.

Nat. Num.

0,0000009	1.000002072
0,0000008	1.000001842
0,0000007	1.000001611
0,0000006	1.000001381
0,0000005	1.000001151
0,0000004	1.000000921
0,0000003	1.000000690
0,0000002	1.000000460
0,0000001	1.000000230

0,00000009	1.000000207
0,00000008	1.000000184
0,00000007	1.000000161
0,00000006	1.000000138
0,00000005	1.000000115
0,00000004	1.000000092
0,00000003	1.000000069
0,00000002	1.000000046
0,00000001	1.000000023

THIS table Mr. *Long* sometimes made use of, for finding the logarithm of any number proposed, and *vice versa*; for instance, suppose you would find the logarithm of 2000, look in the first class of the table (the whole table consisting of eight classes) for the next less to 2, which is, 1.995262315, and against it is 3, which consequently is the first figure of the logarithm sought; again, dividing the number proposed 2, by 1.995262315 the number found in the table, the quotient is 1.002374467, which being looked for in the second class of the table, and finding neither its equal, nor a lesser, add 0 to the part of the logarithm before found, and look for the said quotient 1.002374467 in the third class, where the next less is 1.002305238, and against it is 1, to be added to the part of the logarithm already found, and dividing the quotient 1.002374467 by 1.002305238, last found in the table, the quotient is 1.000069070, which being sought for in the fourth class gives 0, but being sought for in the fifth class gives 2, to be added to the part of the logarithm already found; and dividing the last quotient, by the number last found in the table, *viz.* 1.000046053, the quotient is 1.000023015, which being sought in the sixth class, gives 9 to the part of the logarithm already found; and dividing the last quotient by the new divisor, *viz.* 1.000002072, the quotient is 1.000000219, which being greater than 1.00000115, shews that the logarithm already found, *viz.* 3.3010299 is less than the truth by more than half an unit; wherefore, adding 1, you have *Brigg's* logarithm of 2000, *viz.* 3.3010300.

If any logarithm be given, suppose 3.3010300, throw away the characteristic, then over against these 3..0..1..0..3..0..0 you have in their respective classes, 1.995262315....0...

1.0023

2.002305238.1.000069080.1.000000019966, which multiplied continually, into each other, the product is 2.000000019966, which, by reason the characteristic is 3, becomes 2000.000019966, &c. that is, 2000, the natural number desir'd: The method by which this table is fram'd, is plain from the use of it.

It is obvious, that these classes of numbers are no other than so many scales of mean proportionals; in the first class between 1 and 10; so that the last number thereof, viz. 1.258925412 is the tenth root of 10, and the rest ascending in order are the powers thereof; so in the second class, the last number 1.023292992 is the hundredth root of 10, and the rest in the same manner are powers thereof; so 1.002305238 in the third class is the tenth root of the last of the second, and the rest its powers, &c. Or which is all one, each number in the preceding class is the tenth power of the corresponding number in the next following class; whence it is plain, that to construct these tables, no more than one extraction of the fifth, or sur-solid root for each class is requisite, the rest of the work being done by the common rules of arithmetic; and for extracting the fifth root, you will find more than one very compendious rule in *Phil. Trans.* N° 1210: The process is exactly the reverse of Mr. Briggs's doctrine in Cap. 12. of his *Arithmet. Logarithmica* of Vlacq's edition; and had he been appris'd hereof, it would have greatly eas'd the labour of deducing the logarithms of the first prime numbers, which appear to have cost him so much pains.

Microscopical Observations on the Fibres of the Muscles; by M. Leewenhoeck. Phil. Trans. N° 339. p. 55.

BY M. Leewenhoeck's observations on the membranes, with which the fibrils of the muscles appear to be encompass'd both in the flesh of a whale, cod-fish, salmon and smelt, and also in that of an ox down to a mouse, the appearance is the same in all: Upon viewing thro' a microscope a small piece of whale flesh cut off near the tail of the fish, M. Leewenhoeck judg'd that the fibres thereof were four times as large as those he had formerly observ'd in another piece of whale flesh, taken from another part of the fish; which made him consider, whether the fibres of that part might not be so much the larger, to contribute to its greater strength; upon cutting the said fleshy parts lengthwise, and across the fibres, he discover'd more plainly than before, that each particle, or fleshy fibre

fibre was envelop'd in a fine thin membrane: In order to have a better idea of these fleshy fibres of a whale, he cut a thin slice a-cross, and laid it upon a wetted piece of glass, that the flesh which was very dry, and shrunk, might be swell'd by the moisture, and thereby distended to its natural size on the body of the fish itself; in this state being placed before the microscope, it appeared as represented by A B C D Plate III. Fig. 9, in which the parts lay so close together, that their encompassing membranes, represented by the black lines, were but just discernable, some of which appeared larger than others; these, if attentively view'd, seem'd plainly to be divided into a great many others, also cut transversly, which appear'd to the naked eye no bigger than a grain of common sand; these were crowd'd so close together, that their figure was very irregular, as well as their sizes different; for, tho' each seem'd encompass'd with six others, yet some of them were twice as large as the other.

Having mentioned in a former *Transaction* the slenderness of these fibrils in the flesh of a whale, and judging these, as he said above, to be four times as large, he took a thin slice of the whale flesh formerly mentioned (which he had still kept by him) and after having made it thoroughly wet, he view'd it with the same microscope he had done this of the tail; and it appeared as represented by E F G H Fig. 10. Upon letting the moisture evaporate from these slices, stuck on in this manner to the glass, the particles became much smaller, and the membranes with which each was encompass'd, became very visible, to wit such as were not shrivell'd up; and this was a very entertaining sight; and as often as M. *Leeuwenhoeck* made new incisions, a new object presented itself.

I K L M Fig. 11. represents a small particle of this flesh; these particles seem'd to touch, and to be joined to others; but upon their being dried up, they shrunk in from the membranes round about them; for the membranes themselves could not shrink, because they were all joined to each other: Along these fleshy fibres there run membranes, of the thickness of a hair or better, scarcely distant the breadth of a grain of sand from each other; from these larger membranes other parts were spread, that divided each fibre into a great many fibrils; so that it may be said that each fleshy fibre, no bigger than a hair, is a small muscle encompass'd with its peculiar coat, or membrane: N O P Fig. 12. represents a small piece of these fibres as large as it appeared to M. *Leeuwenhoeck*: He also often
saw

saw some few of these fibres, tho' joined to others, yet but a fourth part of the bigness of those to which they were joined.

Upon moistening again the fibres represented Fig. 11 and 12. (that were dried and shrivell'd up) they became distended in such a manner, as to fill up the spaces between the membranes, and re-assume the shape they had before they were dried; among several little pieces of flesh placed before another microscope, and moistened as above, there was one, whose particles were not separated upon drying; which he supposed owing to the splitting, and tearing asunder of a large membrane that run thro' its middle, as may be seen in QRSTVW Fig. 13, where between ST and V the dried particles remain unseparated; and these being cut a little thicker, appeared also of a darker hue, and had they been sliced still thicker, they would have appeared of a dark red; SW represents the thick membrane dividing this piece, about the thickness of a hair; that at T sent out a branch, and near W is split into two; M. *Leeuwenboeck* supposes, that a great number of blood vessels are spread over this membrane; which, by reason of their smallness, are not visible; for, by these the nourishment is conveyed; between RS and QW the exceeding fine membranes torn from the large ones are visible: It is surprising that such exceeding small fibrils should be found in so huge an animal as a whale; nay, the same as they are in small animals; and that the whole 13th figure is not so big as a coarse grain of sand: This whale was so large, that the upper part of its body yielded 60 *Quarteels* of blubber, or fat, which, allowing 30 *Rotterdam Stopes* (each making about three *English* quarts) to one *Quarteel*, nearly amounts to 24000 pounds weight; besides a great deal of fat about the entrails.

M. *Leeuwenboeck* caus'd a very small piece, consisting only of five fibrils, to be delineated lengthwise, as they were seen thro' the microscope, and represented by ABCDEF Fig. 14. where at A and a little at that place, it is divided into two fibrils; between C and F are to be seen the little membranes, which encompass the fibrils, and which are here torn asunder: M. *Leeuwenboeck* frequently observ'd, with pleasure, these fleshy fibres lengthwise, corrugated or wrinkled, as it were, which he imagined to be the representation of their unbent position; and still more, when the part, to which they belong, is bent together, or brought nearer; but when the muscle is extended, and its antagonist acts, there is not the least

least wrinkle observable in these fibrils: However, all the small inequalities in these fibrils must not be taken for those corrugations, since several of them are only the particles torn off from the membranes, which encompass the fibrils.

G H I K Fig. 15. represents four small fibrils of a piece of flesh of another whale; by the two figures 14 and 15, it is plain, that the diameters of the fibres are as big again in one as in the other; therefore the fibres must be four times as big in Fig. 14. as in 15. Now each fleshy fibre being composed of a great many smaller fibrils, we may imagine each of these inclosed fibres likewise to consist of others of the like nature.

M. *Leewenboeck*, upon viewing several small fibres of ox-flesh, observed, that each of the fibrils in them was encompassed with a thin membrane; but he could not shew these membranes so distinctly to others in cow's flesh as in whale's flesh; because the parts of the former are of a texture much more compact, and close than that of the latter, whence it is they do not shrink so much in drying.

M. *Leewenboeck* is of opinion, that what he has said of the membranes, encompassing the fibres and fibrils of whale-flesh, will likewise hold true in other kinds of flesh; yea, even down to that of a rat, or mouse.

Several Observations on the Frame and Contexture of the Muscles; by M. Muys. Phil. Trans. N^o 339. P. 59.

M. *Muys* observed, that the fleshy fibres of the muscles are composed of other smaller fibres, which he calls fibrils; that these fibrils are of the size of a slender hair, and that 300, or 600 of them may be reckoned in one fleshy fibre, whose diameter is no more than a 24th part of an inch; that each of these fibrils is also made up of more than 300 small transparent *Tubuli*, but so slender, that if a blood globule (which, according to M. *Leewenboeck*, is but the 1,000,000th part of a grain of sand) were divided into 24 parts, one of these could hardly pass thro' those small tubes: He has shewn, that tho' the fleshy fibres of the muscles are joined to the tendons, and tendinous membrane of a muscle; yet that those tendinous fibres are not a continuation of the fleshy ones, as most anatomists suppose; which he proves thus, if by means of a wooden knife, or only by pulling it, you separate the fleshy fibres from the tendon, the extremity of the tendon, to which they were joined, will remain smooth, and even, and not become rugged.

Hav-

Having made several injections of warm water into the crural artery of a lamb of a year old, all the fleshy fibres lost their redness, and became entirely white; the fibres being whitened by this injection, he injected a coloured liquor by the same artery, and then not only the small arteries appeared replete with this tinged liquor, but he also found, that the liquor passed thro' each fibre, either in a serpentine, or indulating manner, or forming several angles, or joined by a great many *Anastomoses*: He also observed, that several small branches of the arteries, which before could not be seen, appeared visibly, spread all round the small fibrils, and tinged with the same colour: Having observed, that the parts of the fleshy fibres, near the extremities of the arteries, appeared tinged with the liquor, he examined them with a microscope, and found the small fibrils filled, and tinged with the same liquor; and yet there was not the least appearance of the liquor in the interstices between the fibrils: Upon injecting by the crural artery another coloured liquor into the muscles, whitened, as before, with water, he saw not only the fibres in some of the muscles, and most part of them in the others, filled with this matter; but upon examining them with a good microscope, he found the fibrils, and even the least *Tubuli*, that compose them, filled and tinged with the same matter; yet the small ramifications of the nerves appeared perfectly white.

From all these observations it follows 1. That the small tubes, which form a fibril, are really hollow, and that the extremities of the capillary arteries open into them, and empty therein a part of their liquor, which is re-conveyed by the veins into the heart. 2. That the blood globules must be divided into an almost infinite degree of smallness, before they can enter, and pass these *Tubuli*: That the blood globules may be so divided, and when so divided, pass thro' the small *Tubuli*, is evident from the redness of the fibres and fibrils of animals, which have a red flesh; and this farther appears from M. *Leeuwenhoek*, who says, that these globules divide themselves after this manner, in order to pass thro' the last extremities of the capillary arteries of the brain; as also from these globules being exceeding soft, and easily separable, as M. *Muys* has evinced by arguments grounded on very curious observations: M. *Muys* found out a way of separating the salts from the blood without any chemical analysis, and without making them undergo any change; and of forming them into crystals, visible without a microscope.

An Account of several Observations made in New England in 1712; by Dr. Mather. Phil. Trans. N° 339. p. 62.

THE Dr. confirms the opinion of there having been in the antedeluvian world, giants, or men of very large and prodigious statures, by the bones, and teeth of some large animals, found in *Albany in New England*, which for some reasons, he judges to be human; particularly, a tooth, which was a very large grinder, weighing four pounds and $\frac{3}{4}$, with a bone, supposed to be a thigh-bone 17 foot long; he also mentions another tooth, broad and flat like a fore-tooth, four fingers in breadth; the bones crumbled to pieces in the air after they were dug up; they were found near a place called *Cluverack*, about 30 miles on this side of *Albany*; he then gives a description of one, which he compares to the eye tooth of a man; he says it had four prongs or roots, flat, and somewhat worn on the top; as it stood upright on its root it was six inches high wanting $\frac{1}{4}$, and almost 12 inches in circumference; it weighed two pounds four ounces *Troy*; there was another near a pound heavier, found under the bank of *Hudson's river*, about 50 leagues from the sea, a great way below the surface of the earth, where the ground was of a different colour, and substance from the other ground, for 75 foot long, which is supposed to be owing to the rotting of the body, to which these bones and teeth, as the Dr. supposes, did once belong: It were to be wished, that the Dr. had given an exact figure of these teeth, and bones.

The Dr. conjectures the *Sbittim* wood, mentioned in sacred writ to have been made use of for the ark, &c. and said not to be subject to putrefaction, as most other woods are, to be the black *Acacia*; and that the *Gopher* wood was the *Juniperus arbor tetragonophyllos*, frequent in the *East-Indies*, &c. He observes, that the *Indians* often perform very considerable cures with their plants, of which there is a great variety, and differing from the *European*; he instances in some of them; as a plant efficacious in curing inflammations, whence they call it *Antierisypelas*; it grows plentifully in the woods; a chemical oil extracted from it, taken internally performs wonders in absorbing scorbutic salts; another plant, that goes by the name of partridge-berries, is excellent in curing the dropsy; a decoction of the leaves, being drank several days as a tea, causes a discharge of a vast quantity of urine, as long as the disease lasts; after which it may be drank without observably provoking urine; gouty persons drink it with benefit: The root called the bleed-

bleeding-root cures the jaundice in five or six days; and another, whose name he omits, cures gangrenes; he mentions another specific for the bite of the rattle snake, and another for quinries, or sore throats; a plant called by the *Indians Taututtipang*, is infallible for curing the *Lues venerea*, by using the root in a decoction, and drinking half a pint of it; a cataplasm of the same root, bruised and applied to the ulcers, does also cure them; a decoction of the root of a thistle, called the *Boar-thistle*, very short and prickly, and of a root, called the *Cancer-root*, and a sort of *Devil's-bit*, is a cure for the *King's-evil*, tho' very far gone, by drinking a small quantity every day, and applying the bruised roots to the scrophulous tumors.

As to their birds, they have several of the same species with ours in *England*; he mentions very large wild turkeys, some weighing 50, or 60 pounds, but whose flesh is very tough and hard; he takes notice of a very large eagle with a big head, soaring very high, as all of that *genus* do; as to the itinerants, or birds of passage, there are vast flights of pigeons, which come and depart at certain seasons; and he imagines they repair to some undiscovered satellite, accompanying the earth at no very great distance.

As to antipathies, and the force of imagination; he says, that a gentlewoman of his neighbourhood would swoon away upon seeing any one cut their nails with a knife, but if done with a pair of scissars, it would have no effect upon her: That the wife of a certain person would vomit, upon seeing her husband take a vomit, the latter being noways affected, but obliged to take a fresh emetic: That a woman with child longing for pease, but refusing to gratify her desire, for fear of a sort of bug, with which most of their pease were infested at that time; this woman's child, when born, had an excrescence on the fore-head, resembling one of those pease, with a black speck, as the buggy pease had, which some time after dried away, and was shelled out, as the bugs are observed to leave the husk of the pea.

Dr. Matber observes, that the *Indians* have no other division of time, than by sleeps, moons, and winters: Tho' the *Indians* have not distinguished the stars into constellations, yet it is observable, that they call the stars of *Ursa major*, *Pankunawaw*, that is, the *Bear*, and this long before they had any communication with *Europeans*: He says, that there is a tradition among them, that in *November 1668*, a star appeared below the body of the moon, within its horns: The *evening glade*, first taken notice of by Dr. *Childrey*, is constantly observed there in *February*, and a little before, and after that month; and he thinks,

the cause of that appearance must be sought for above the atmosphere: On the 2d of *January* in a clear sky, but very cold, the sun was from 10 o'clock for near three hours after, attended with four *Parhelia*, in the middle of which were two rain-bows: About six weeks after, in a day much colder, than used to be at that time of the year, the air being somewhat hazy, a little after one o'clock, there were seen four mock-suns for about half an hour; he observes, that these appearances with them are of great variety, each usually differing in some respect from the other.

The Dr. next gives an account of a strange discovery in a dream of the murder of a person in *England* to his brother *Joseph Beacon*, at that time at *Boston*, the wounded person appearing to him with the wound on his head; the information by the dream was on the 2d of *May* 1687, about five o'clock in the morning, on the very day his brother died at *London* of the wounds he had received the *April* before; of which misfortune his brother *Joseph Beacon* neither had, nor could have any notice, till the next communication by shipping, towards the latter end of *June* following, when he had a letter of his brother's death, and the cause of it, according to his dream.

Then the Dr. treats of the *rattle-snake*, hinting at the occasion of its name, from the rattles in its tail, in which he says there are sometimes 20 of those loose rings, tho' he does not give into the common opinion, that one is added every year: Next follow these observations, *viz.* that the more northerly they travel, these snakes are less numerous, as well as less venomous; and that none are seen to the north of *Merimac* river, which is about 40 miles north of *Boston*; here he relates a story, as he says, constantly affirmed by the *Indians*; *viz.* that these snakes frequently lie coiled at the bottom of a large tree, with their eyes fixed on some squirrel above in the tree, which tho' seeming by his cries, and leaping about to be in a fright, yet at last runs down the tree into the jaws of this devourer; then he relates, that the winter abode of these snakes is in the clefts of inaccessible rocks, from whence in the spring they come forth, a sunning themselves, at first very feeble, which is the only time of destroying them; and then the *Cystis* or gall-bladder in these snakes is full of an acid, azure-coloured juice, which they squeeze out into a glass, but it is so spirituous, that if the glass be not immediately stopped up, it will soon evaporate; they mix this liquor with a proper quantity of powdered chalk, or *Indian* meal, and use it as a medicine against the venomous bite of this snake; some have called it *Trochisci Connecticutiani*, from the *Connecticut* colony; it is observable, that when the summer heats

come

come on, the snakes have no longer this azure liquor in their gall-bladders, in which there is only found a black thick sediment of no known use; at which time they suppose the above-mentioned spirituous juice is conveyed to, and lodged in their gums, and so emitted by the hollow of the teeth into the wound, when they bite, after having received another digestion, and higher exaltation, by passing thro' several strainers, and glands, before it arrives at the gums; as an instance of the virulence of this liquor, he tells us, that a traveller killing one of these snakes, suffered the enraged dying viper, to bite the end of his switch, with the lashes of which he had disabled it, and afterwards a fly causing an itching in one of his temples as he rode on, he rubbed it with the other end of the switch, which immediately caused his whole head to swell very much, the poison, as he supposes, permeating the whole length of the switch. He adds another account as to the penetrating quality of this poison; a certain person provoking a rattle-snake to bite the edge of a broad ax he had in his hand, the colour of the steeled part, that was bitten, was immediately changed, and at the first stroke he made with it, the discoloured part was broken out, leaving a gap in his ax: As to the *Trachises* made of the gall, he says it is a iudorific cordial, and so good an anodyne, that some take three or four grains of it to compose them to rest after travel; it is good in all fevers, especially, malignant ones; it is an infallible remedy for obstructions incident to women upon catching cold in child-bed; and being taken in a proper quantity 12 hours before the fit, it infallibly cures a quartan ague; the dose is 14 grains more, or less, in any vehicle, according to the circumstances of the patient.

The Dr. next gives an account of the effects of thunder and lightning, very common with them; which from its frequent destroying animals, without any visible hurt on the external parts, he compares to the *Jewish* punishment of pouring melted lead down the throats of condemned criminals, which they called *Combustio anime*; tho' he likewise observes, that some have had their hair singed, with marks on their skin, resembling those made by small shot; and some their bones made limber like a gristle; the captain of their castle was found dead in his bed, after a storm of lightning, without any apparent hurt; here he relates a passage of which an account was given in a former *Transaction*, viz. that on *July* the 24th, 1681, a ship whereof one Mr. *Lad* was master about 100 leagues from *New England* in *Lat.* 38, met with a violent storm of thunder, which did the ship much damage; at which time a bituminous matter fell on the

the ship, burning with such violence, as not to be extinguished with water, till it was all burnt out, and smelling strongly like fir'd gunpowder; and when they came to observe the stars at night, they found the polarity, or direction of their sea compasses to be changed, the north point turned to the south, and they continued so to do for the rest of the voyage for 1000 leagues; he adds further, that one of these compasses continued to do so still, as low down as 1712: The Dr. queries, whether this may be accounted for by Mr. Boyle's experiment of heating a loadstone red hot, and whether by altering the position in which it was cool'd, its polarity could be changed? which some may say, might happen to this needle, supposing it was made red hot, and turned upon its center in a storm.

From thunder the Dr. proceeds to earthquakes, which, tho' he says, they have not done with them at *Boston*, the mischiefs frequent in *Sicily*, *Italy*, &c. yet they have had several very sensible and dreadful ones: In 1663 they had six or seven violent shocks in the space of three days; a town lying on the river *Connecticut* has had scores of them in a year, for several years together; the *Indians* affirm, that several rivers have not only been stopped in their course, and diverted, but some entirely swallowed up by earthquakes; he further adds a passage out of *Joselin*, who dwelt in the neighbourhood, that in 1670, at a place call'd *Kenebunch*, near the side of the river, a piece of clay ground was thrown up over the tops of high oaks, growing between it and the river, which it thereby stopp'd, and left a hole in the place, from which it was thrown 40 yards square, &c. Next as to storms of hail the Dr. relates, that they have had very extraordinary ones, insomuch that they have lain three or four foot thick on the ground, some as big as hens eggs, and others five times as big; he mentions, as an accident sometimes happening to them in the winter, that it has rained plentifully, and at night frozen so extremely, that the weight of the icicles has broken the boughs of the trees, and very often split their trunks: Tho' they have not those hurricanes to which the *Caribbee* islands are subject, yet they have had whirlwinds, or gusts drive along a particular narrow tract for divers miles together, with an irresistible violence, so that if their towns had stood in the way, they must undoubtedly have been destroyed; of these, he says, a thick dark, small cloud has arisen with a pillar of light in it, of about 8 or 10 foot in diameter, and past along the ground in a tract no wider than a street, horribly tearing up trees by the roots, blowing them up in the air like feathers, and throwing up stones of a great weight to a considerable

able height in the air, thus demolishing every thing in its passage; and the noise of this cloud was so great all the while, that that of the mischiefs done by it was thereby quite drowned: There is a huge rock in the middle of *Merimack* river, on the top of which are a great number of pits, made exactly round, like barrels, or hogsheds, of different capacities, some so large as to contain several tuns; the natives know nothing of the time they were made; but the neighbouring *Indians* have been wont to hide their provisions in them, in their wars with the *Maqua's*; they appear plainly to be artificial.

The Dr. affirms it is no rare thing with them to have an aged gentlewoman see many more than 100 of her offspring; he mentions one woman that had 23 children, of which 19 lived to man's estate; another that had 27; another 26; and another 39: Here he gives several instances of persons living to upwards of 100 years of age; one *Clement Weaver* lived to 110, and his wife was upwards of 100; this man to the last year could carry a bushel of wheat to the mill, upwards of two miles: He relates the case of an old man upwards of 100 that had lost the memory of several of the latter years of his life, but very well retained the remembrance of what passed in his younger days; and yet it does not appear that they observed any regularity, or method in their manner of diet, exercise, &c.

An Account of procuring the Small-pox by Incision, or Inoculation, as practis'd at Constantinople, in 1713; by Dr. Timoni. Phil. Trans. N° 339. p. 72.

THE Dr. observes, 1. That the *Circassians*; *Georgians*, and other *Asiatics*, had for upwards of 40 years before, introduced this practice of procuring the small-pox by a sort of inoculation, among the *Turks* and others at *Constantinople*; that tho' at first the more prudent were very cautious in the use of this practice, yet the happy success it was found to have on several persons, had put it out of all suspicion and doubt, the operation having been performed on persons of all ages, sexes and temperaments, and even in the worst constitution of the air, without any one being found to die of it; whereas the small-pox seizing the patients the common way, proved very mortal; half of those affected therewith dying of them; this the Dr. attests from his own observation. 2. That such as had this inoculation practis'd upon them, were subject to very slight symptoms, some being scarce sensible, that they were ill or sick, and which recommends it to the fair sex, it never left any scars, or pits in the face.

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The method of the operation is thus; choice being made of a proper contagion, the matter of the pustules is to be communicated to the person proposed to take the infection, whence it has metaphorically the name of *Infection* or *Inoculation*; and for this purpose, they pitch upon some boy or young lad, of a sound, healthy temperament, that is seized with the common small-pox (of the distinct not confluent kind) on the 12th or 13th day from the beginning of his sickness; they prick with a needle the tubercles (chiefly those on the shins and hams) and squeeze out the matter coming from them into some convenient glass vessel, or the like, to receive it; and it is proper to wash and clean the vessel first with warm water; a convenient quantity of this matter, being thus collected, it is to be covered down close, and kept warm in the bosom of the person that carries it, and as soon as possible brought to the expecting future patient, who being in a warm chamber, the operator is to make several little wounds with a needle in one, two, or more places of the skin, 'till some drops of blood ensue, and immediately he is to pour out a few drops of the matter contained in the glass, and mix it well with the blood as it issues out; a single drop of the matter is sufficient for each place that is pricked; these punctures are made indifferently in any of the fleshy parts, but they succeed best in the muscles of the arm, or *Radius*; the needle is to be a three edged surgeon's needle; it may likewise be performed with a lancet; the manner is to run the needle transversely, and rip up the skin a little, that there may be a convenient dividing of the part, and the mixing of the matter with the blood be more easily performed, which is done either with a blunt style, or an ear-picker; the wound is covered for a few hours with half a walnut-shell, or the like concave vessel, and bound over, to prevent the matter being rubbed off by the cloaths; the patient is to take care of his diet: At *Constantinople* the custom is to abstain entirely from flesh, and broth for 20 or 25 days: This operation is performed, either in the beginning of winter, or in the spring: Some, for caution, order the matter to be brought from the patient by a third person, lest any infection should be conveyed by the cloaths of the operator; but this is not material.

As to the process of this matter, in respect of the idiosyncrasy; the small-pox begin to appear sooner in some than in others; in some with more severe, in others with milder symptoms, but with a happy issue in all: At *Constantinople* the efflorescence commonly begins at the end of the seventh day, which seems to favour the doctrine of *Crises*: It was observed, in a year when the common
small-

small-pox were every year mortal, that those by insition were also attended with severer symptoms; of 50 persons on whom the insition was made almost in the same day, there were four in whom the eruption was too sudden, the tubercles more numerous, and the symptoms worse; there was some suspicion, that these four had caught the common small-pox before the insition was made; it is enough for the present purpose, that they all recovered after the insition; in those four the small-pox nearly resembled the confluent kind; at other times those by inoculation are distinct, few and scattered, commonly 10 or 20 break out; here and there one has but two or three, and few have 100; there are some in whom no pustule arises, but in the places where the insition was made, which swell up into purulent tubercles; yet these have never had the small-pox afterwards in their whole lives, tho' they have cohabited with persons infected with them: It is to be observed, that a considerable quantity of matter runs for several days from the place of the insition: The pustules arising from this operation are dried up in a short time, and fall off, partly in thin skins, and partly contrary to the common sort, they vanish by an insensible wasting; the matter is hardly a thick pus, as in the common sort, but a thinner kind of sanies, whence they rarely pit, excepting at the place of insition, where the cicatrices left are not to be worn out by time, and whose matter comes nearer the nature of pus: If any apostheme break out on any (which infants are most subject to) yet there is no danger, for, it is safely healed by suppuration; if any other symptom happen, it is easily cured by the common remedies: It is to be observed, that they scarcely ever make use of the matter of the inoculated pox for a new insition; if this inoculation be made on persons, who have had the small-pox before, they find no alteration, and the places pricked presently dry up, excepting in an ill habit of body, where possibly a slight inflammation and exulceration may happen for a few days.

The Dr. never observed any mischievous accident from this insition; and tho' such reports had been sometimes spread among the vulgar, yet having gone on purpose to the houses whence such rumours had arisen, he found the whole absolutely false; he had been an eye-witness of these operations for eight years, and to give a proof of his diligence in this disquisition he gives the two following accounts: In a certain family a boy of three years of age was afflicted with the falling sickness, the king's-evil, an hereditary *Lues*, and a long *Marasmus*; the parents were desirous to have the insition made upon him; the small-pox were

thrown off with ease, but about the fortieth day he died of his *Marasmus*: In another family a girl of three years of age was troubled with the like fits, was stumous, and had an hereditary *Lues*, and laboured under a colliquative looseness for three months; the operation was performed on this child, she came off very well with the small-pox, which were all over the 15th day, but on the 3d she died of her looseness, which had never left her all the time: The Dr. owns, he never maintained the inoculation as a *Panacea*, or cure for all diseases, nor does he think it proper to be attempted on persons likely to die.

Some Theorems on the rarity of Matter, and the tenuity of its Composition; by Dr. John Keill. Phil. Trans. N° 339. p. 82. Translated from the Latin.

THE Dr. supposes, that all matter is divisible *in infinitum*, and that it can assume any form or figure, and be reduced to any degree of tenuity. *Lemma.* Any quantity of matter being given, there may be formed of it, or any part thereof a concave sphere, whose semi-diameter shall be equal to a given right line; let the particle of matter be a^3 and the given right line b ; let the *Ratio* of the periphery of a circle to its *Radius* be as p to r ; let the semi-diameter of the concavity be call'd x , and the thickness of the pellicle or shell, encompassing the spherical concavity will be $b - x$, and the cylinder circumscribing a sphere, whose *Radius* is b , will be $\frac{p \times b^3}{r}$; whence the sphere inscribed within the cylinder will be $\frac{2pb^3}{3r}$; by the same reason the sphere whose *Radius* is x will be $\frac{2px^3}{3r}$; whose difference $\frac{2pb^3}{3r} - x^3$ is to be put equal to the spherical *Lamella*, or shell, or given particle of matter; that is, $\frac{2pb^3}{3r} - x^3$ will be $= a^3$, or $b^3 - x^3 = \frac{3ra^3}{2p}$, whence $x^3 = b^3 - \frac{3ra^3}{2p}$, and $x = \sqrt[3]{b^3 - \frac{3ra^3}{2p}}$; consequently, the thickness of the spherical *Lamella*, or $b - x$ will be $= b - \sqrt[3]{b^3 - \frac{3ra^3}{2p}}$: By the same reason may be formed of a given quantity of matter concave cubes, concave

concave cylinders, or concave bodies of any other figure, whose sides shall be equal to a given right line.

Theorem. 1. *Any Quantity of matter however small being given, and any finite space however large being likewise given; which suppose a cube that shall circumscribe Saturn's orb; it is possible, that the matter of that very minute particle may be diffus'd thro' all that space, and fill it in such a manner, as to leave therein no vacuity, whose diameter shall exceed a given right line.*

Let the given space be a cube, whose side is the right line AB , Plate IV. Fig. 1. to wit, equal to the diameter of Saturn's orbit, and let a particle of matter be given, whose quantity is b^3 , and let the given right line (which the diameter of the pores or vacuities should not exceed) be d ; let the right line AB be supposed to be divided into parts, equal to the right line d , whose number will be finite, seeing the right line AB is not supposed to be infinitely great, nor the right line d infinitely small; let that number be n , that is, let $nd = AB$, consequently $n^3 d^3$ will be equal the cube of the right line AB ; let the given space be likewise supposed to be divided into cubes, each of whose sides shall be equal to the right line d , and the number of the cubes will be n^3 , and let these cubes be represented by the spaces $efgh$ in the figure; moreover, let the particle b^3 be supposed to be divided into parts, whose number is n^3 , and let one of these particles be placed in each cubical space, and by this means the matter of b^3 will be diffus'd thro' all that space; besides each particle of b^3 , being placed in its proper cell, as it were, may be formed into a concave sphere, whose diameter shall be equal to the given right line d ; whence it will happen, that each sphere will touch that next to it, and the given particle of matter b^3 , however small, will fill the given space in such a manner, as to leave therein no vacuity, whose diameter shall exceed the given right line d . Q. E. D.

Corol. Hence a body may be given, the matter of which, if reduced into a space absolutely full, that space may be any given part of the former magnitude.

Theor. 2. *There may be two bodies, equal in bulk, whose quantities of matter may be any how unequal, and have any given Ratio to each other, yet the sums of the pores, or vacuities*

ties within the bodies, shall nearly approach to a Ratio of equality: Or in the Cartesian stile; all the space possess'd by a subtile matter within the pores of one body, may be almost equal to the space occupied by the like matter within another body, tho' the proper matter of one body may be 10,000, or 100,000 times greater than that of the other; and the bodies may be equal in bulk.

Ex. gr. Suppose a cubic inch of gold, and a cubic inch of common air not condens'd, it is certain, the quantity of matter in the gold, will exceed that of the air about 20,000 times, yet it is possible, that the spaces in the gold either absolutely empty, or fill'd with subtile matter, may be almost equal to the spaces in the air, either empty, or replete only with a subtile matter.

Let A and B Fig. 2. be two bodies of equal bulk, and let both *e. g.* be a cube of one inch; and let the body A be 10,000 times heavier than the body B, whence the quantity of matter in the body A will be 10,000 times greater than that in the body B; now, let us suppose, the quantity of matter in A to be reduced into a space absolutely full, which let be the 100,000 part of a cubic inch, the possibility of which appears from *Corol.* of preceeding theorem; whence, since the quantity of matter in A is 10,000 times greater than that in B, the matter in A if reduced into a space absolutely full, will possess only the 1,000,000,000th part of a cubic inch; consequently, the remaining parts 999999999 will be either absolutely empty, or only replete with some subtile matter, such as the *Cartesians* suppose: Moreover, since the quantity of matter in A possesses only the 100,000th part of an inch, there will be in the body A 999999 parts, either empty or replete with a subtile matter; that is, by reducing the fraction to the denominator of the former, the vacuities in A will be 999990000; consequently the vacuities in A will be to the vacuities in B, as the number 999990000 to the number 999999999; which numbers are almost in a *Ratio* of equality to each other; for, their difference bears but a small *Ratio* to the numbers themselves; consequently, the spaces in the two bodies A and B, that are either empty, or fill'd only with a subtile matter, having the same *Ratio* to each other as these numbers, are likewise almost in a *Ratio* of equality. Q. E. D.

From the properties of diaphanous or transparent bodies, it is very evident, that all bodies are of a considerable rarity, that

that is, contain an exceeding small quantity of matter in proportion to their bulk; for, the rays of light are diffus'd within glass or water, equally as in air (whatever side of the diaphanous body be expos'd to the light) in right lines; consequently, there is always in these bodies a rectilinear pore, thro' which light may pass, extending from the least assignable part of a diaphanous body, to any other part of the same body; but this could not possibly happen, unless the matter of the diaphanous body had a very small *Ratio* to its bulk; and perhaps the quantity of matter in glass has not a greater *Ratio* to its bulk, than a grain of sand to that of the whole terrestrial globe; and that this is not impossible, has been shewn above; hence since gold is not eight times denser than glass, its quantity of matter shall likewise have a very small *Ratio* to its bulk: Hence the reason may be assigned, why the magnetic effluvia do almost with the same facility pervade both dense gold, and rare air: From these propositions, and from the great velocity of light, the reason may likewise be assigned, why the rays of light, proceeding from several objects, and passing thro' a small hole, do not mutually hinder each other, but continue in their motion in the same streight line; which can hardly be explain'd by the motion, or impulse of a fluid, forming a *Plenum*; for, any body impell'd by several powers at the same time, in different directions, will receive one determinate direction only, compounded of them all.

Observations made at Rome, Nov. 21. in the Morning 1713, on the Occultation of the Star τ at the Root of the Northern Horn of the Bull, under the Moon's Disk; as also an Eclipse of the Moon presently ensuing thereon; together with some Emerfions of the innermost Satellite out of Jupiter's Shadow; by S. Bianchini. Phil. Transl. N^o 340. p. 88. Translated from the Latin.

H. p. Mer.

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12 54 34

THE star (call'd by *Bayer* τ *Tauri*) approaches the moon's limb, being observ'd by a telescope of 12 palms.

It was covered by that part of the moon's limb, which is almost in the middle between the *Macule* of *Aristarchus* and *Galileus*; a diurnal parallel describ'd from the center of the moon appears more southerly than the star τ by 7 and $\frac{1}{4}$ parts of the micrometer, whereof the moon's diameter subtends 37; therefore, the declination of the star τ

is

H. p. Mer.	is more northerly than the apparent declination of
H. ' "	the moon's centre by about $5\frac{1}{2}$ of a great circle.
14 0 14	<i>Sirius</i> in the meridian; whence the times are verified.
14 32 57	The star γ , which had emerged some minutes out of the moon's limb, in its diurnal revolution preceeds the moon's western limb by $33''$ of time, as also the moon's center by $103''$ or $1' 43''$ of time.
14 42 50	It preceeds the moon's limb by $48''$ and its center by $1' 58''$ of time.
14 50 37	The difference of the right ascension of the star and limb is $1' 03''$, and of the center of the moon and the same star is $2' 13''$.
15 0 0	The <i>Penumbra</i> on the moon's limb, which before was more diluted, gradually becomes more dense.
15 2 20	The <i>Penumbra</i> becomes more observable, but the true shadow does not yet appear.
15 4 20	The moon, in that part of its limb next <i>Macula Schiccardi</i> , begins to enter into the true shadow.
15 5 21	The true shadow now covers one part, whereof the moon's diameter is 37 in the micrometer.
15 7 20	Two such parts are obscured.
15 16 20	$3\frac{1}{2}$ such parts are obscured.
15 31 20	$3\frac{3}{4}$ such parts obscured.
16 12 0	$4\frac{1}{2}$ such parts obscured.
16 17 20	$4\frac{3}{4}$ obscured as before.
16 50 20	$4\frac{3}{4}$ parts obscured.
16 54 35	The former limb of <i>Tycho</i> begins to emerge.
16 56 9	Now <i>Tycho</i> quite emerged.
17 13 30	$4\frac{3}{4}$ parts of the moon's diameter obscured.
17 27 45	The true shadow gone off the moon's limb, in the middle of the diameter drawn between <i>Aristarchus</i> and <i>Plato</i> .

This observation is the more valuable, as the occultation of the star γ happened so near the sun's opposition, that from thence the sun's place among the fixed stars may be accurately determined.

September $11^h 38' 20''$ P. Mer. 1713 at Rome, *Jupiter's* innermost satellite begins to emerge, opposite to the space between the two belts of *Jupiter*; the observation was made with a telescope of S. *Chiarelli's*, of 40 Roman palms: At $8^h 44'$ the third

third satellite appear'd in conjunction with the fourth in such a manner, that both seem'd to be but one; they were distant from *Jupiter's* center about 5 and $\frac{1}{2}$ diameters of *Jupiter*: At $9^h 4'$ they appear'd separate; the fourth satellite appear'd in an inverted position, a little more depress'd than the third, and at a somewhat greater elongation from *Jupiter*; wherefore, it was more northerly than the third satellite.

September $\frac{11}{12}$ at $10^h 36' 23''$ the first or innermost satellite begins to emerge out of the shadow, being observ'd with *S. Campani's* telescope of 25 palms.

November $\frac{11}{12}$ at $7^h 32' 22''$ the first satellite begins to emerge, being observ'd thro' *Chiarelli's* telescope of 40 palms; afterwards at $7^h 46'$ the same night the first and second satellite were the nearest, and at $7^h 53'$ they were so very near, as scarce to be distinguish'd from each other.

December 9. N. S. or Nov. 28. O. S. at $5^h 45' 45''$ the first satellite begins to emerge out of *Jupiter's* shadow.

December 21 O. S. at $5^h 50' 22''$ the first satellite is seen again, beginning to emerge out of the shadow.

From these observations accurately calculated, it is plain, that the second equation, which we suppose to arise from the progressive motion of light, does necessarily take place; for, after 57 revolutions of the innermost satellite, in which *Jupiter* receded from the earth more than the *Radius* of the *Orbis Magnus*, the last eclipse was seen almost nine minutes later, than it should have been according to the tenor of the first observation; which agrees with *Cassini's Hypotheses*: From the same observations it is also plain, that the motion of *Jupiter's* innermost satellite is somewhat swifter than by *Cassini's* very accurate tables; yet that inconsiderable error seems scarce to exceed two minutes of time in each revolution of *Jupiter*, or in 12 years, by which the heavens anticipate *M. Cassini's* calculation; but with this correction, the agreement will be pretty accurate.

An Account of the Rain which fell at Upminster in Essex, for 18 Years, with Remarks upon that of 1714; by Mr. Derham. Phil. Trans. N° 341. p. 130.

THE year 1713 having been so remarkably dry, that ponds about *Upminster* were for the most part dried up, and the springs generally either very low or quite failing, *Mr. Derham* made an extract (out of his registers of the weather, &c.) of the quantity of rain which fell at *Upminster* for 18 years: the particulars of which for every year may be seen in the following table;

table; in one column of which the weight of the rain in pounds Troy and centesimals of pounds may be seen; in the other its depth in inches and centesimals of inches, or what height it would have been of, had it not been imbibed by the earth, or lessened by exhalations, but have been suffered to stagnate on the ground.

Among the dry years 1704 was complained of for one, which the news-papers reported to have been so considerable at *Venice*, that they were forced to fetch their water in barks five leagues off, as far as the *Brenta*, so that publick prayers were put up for rain; yet several other years were drier than that at *Upminster*; but among them all none comparable to 1714, in which the whole quantity of rain was no more than 561. 95 hundred parts, or 11 inches 19 hundred parts; whereas the least quantity of any of the preceeding 18 years exceeded 15 inches in depth.

What effects this drought had on the bodies of animals Mr. *Derham* leaves others to judge; it is well known how contagious and fatal a distemper raged not only among the black cattle in *England*, but in several other parts of *Europe*; and Mr. *Derham* observed, that the itch was epidemical among the poorer sort, at the beginning of the year; that the measles were very common for some parts of the year; and that pleurifies, and malignant fevers infested a great many, especially in the summer months; but how far these distempers might be owing to the dry season, Mr. *Derham* leaves others to determine.

A table

A table of rain which fell at *Upminster* from 1697 to 1714.

Year	Weight.		Depth.	
	L.	Cent.	Inch.	Cent.
1697	77	60	15	52
1698	122	32	24	46
1699	75	54	15	41
1700	95	13	19	03
1701	93	45	18	69
1702	101	89	20	38
1703	119	94	23	99
1704	79	02	15	81
1705	84	62	16	93
1706	121	43	24	29
1707	81	55	16	31
1708	96	09	19	22
1709	132	82	26	56
1710	91	84	18	37
1711	118	02	23	60
1712	118	78	23	76
1713	115	80	23	16
1714	55	95	11	19

To compare with these, the quantity of rain, and dissolved snow, which had fallen at the *Observatory* at *Paris* for 23 years together, according to the accurate observations of *M. De la Hire*, an extract on that account was made out of the *Memoirs of the Royal Academy* of sciences; and that the comparison might be the more just, the *French* measure is reduced to the *English*; but it is to be observed, that the diversity of stile makes the years not exactly the same, tho' as to this matter the difference may seem very inconsiderable.

Anno.	French		English	
	Inch.	Lin.	Inch.	Cent.
1689	18	11 $\frac{1}{2}$	20	23
1690	23	3 $\frac{1}{2}$	24	87
1691	14	5 $\frac{1}{2}$	15	40
1692	22	7 $\frac{1}{2}$	24	14
1693	22	8	24	18
1694	19	9	21	07
1695	19	7 $\frac{3}{4}$	20	96
1696	19	5 $\frac{1}{2}$	20	76
1697	20	3	21	60
1698	21	9	23	20
1699	18	8 $\frac{1}{2}$	19	93
1700	20	0 $\frac{1}{2}$	21	38
1701	21	4 $\frac{1}{2}$	22	78
1702	16	4	17	42
1703	17	4 $\frac{1}{2}$	18	51
1704	19	10 $\frac{1}{2}$	21	20
1705	13	10 $\frac{1}{2}$	14	82
1706	15	3 $\frac{1}{2}$	16	32
1707	17	11	19	11
1708	18	3 $\frac{1}{2}$	19	51
1709	21	9 $\frac{1}{2}$	23	21
1710	15	8 $\frac{1}{2}$	17	10
1711	25	2	26	84

An Account of several extraordinary Meteors, or Lights in the Sky; by Dr. Halley. Phil. Transf. N^o 341. P. 159.

THE theory of the air seems now to be perfectly well understood, and its different densities at different altitudes to be sufficiently defined both by reason and experiment; for, supposing the same air to possess spaces reciprocally proportional to the quantity of the superior or incumbent air, the Dr. has elsewhere proved, that at the height of 40 miles, the air is rarer about 3000 times than at the surface of the earth; and that the utmost height of the atmosphere, which reflects light in the *Crepusculum*, is not full 45 miles; notwithstanding which, it is still manifest, that some sort of vapours, and those in no small quantity, arise nearly to that height; an instance of this may be given

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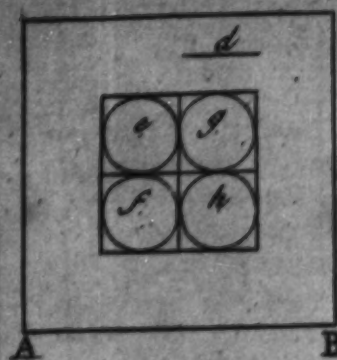


Fig. I.

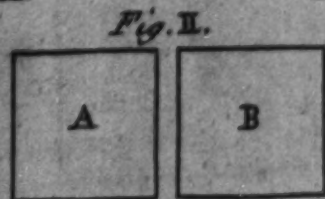


Fig. II.

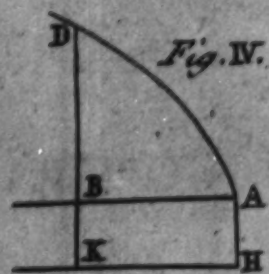


Fig. IV.

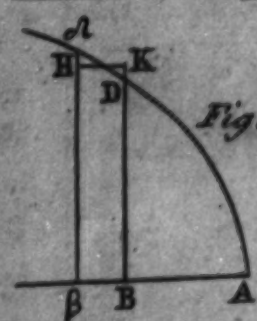


Fig. V.

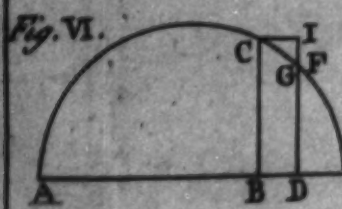
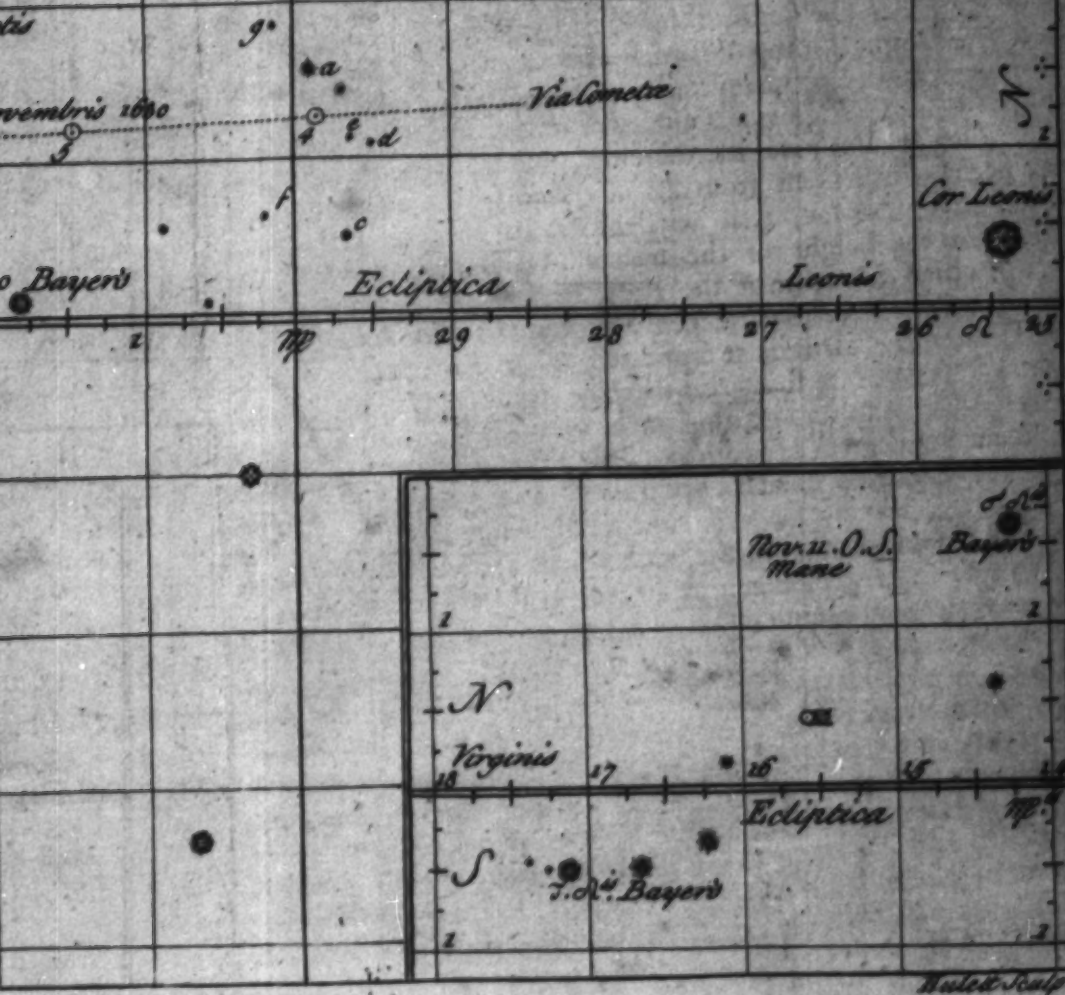


Fig. VI.

G. Smith del.



Fig. III.



given in the great light, of which there was an account in a former *Transaction* by Dr. Wallis, which was seen in very distant counties almost over all the south part of *England*; of which tho' the Dr. could not procure so particular an account, as was requisite to determine its height, yet from the distant places it was seen in, it could not but be several miles high: In like manner that meteor, seen in 1708 on the 31st of *July* between nine and ten o'clock at night, was evidently between 40 and 50 miles of perpendicular height, and as near as Dr. Halley could guess, over *Sheerness*, and the *Buoy on the Nore*; for it was seen at *London* moving horizontally from E. by N. to E. by S. at least 50 degrees high, and at *Redgrave* in *Suffolk*, on the *Tarmouth* road, about 20 miles from the east coast of *England*, and at least 40 miles to the eastward of *London*, it appeared a little to the westward of the south, suppose S. by W. and was seen about 30 degrees high, sliding obliquely downwards: So that the Dr. concludes, it was not many miles more westerly than *Redgrave*, which, as is said above, is upwards of 40 miles more easterly than *London*; suppose it, therefore, where perpendicular, to have been 35 miles east from *London*, and by the altitude it appeared in at *London*, viz. 50 degrees, its tangent will be 42 miles, for the height of the meteor above the surface of the earth; which is rather of the least, because the altitude of the place is rather more than 50 degrees; and the like may be concluded from the altitude it appeared in at *Redgrave*, near 70 miles distant, tho' at this great distance it appeared to move with an incredible velocity, darting in a very few seconds of time, for about 12 degrees of a great circle from north to south, and being very bright at its first appearance; at the end of its course it died away, leaving for some time a pale whiteness in the place, with some remains of it in the track wherein it had gone, but no hissing sound, or explosion were heard, as it passed.

It may deserve consideration, how so great a quantity of vapour should be raised to the very top of the atmosphere, and be there collected, so as upon its accension, to diffuse a light to a circle of upwards of 100 miles diameter, not much inferior to that of the moon, so that one might see to take up a pin from the ground, in the otherways darkest night: It is hard to conceive what sort of exhalations should rise from the earth, either by the action of the sun, or subterraneous heat, that should thus surmount the extreme cold, and rarity of the air in those upper regions, but the fact is indisputable, and therefore, requires a solution.

Like to this, but much more considerable was that famous meteor, which was observed to pass over *Italy* on the 21st of *March* O. S. 1676, about an hour and three quarters after sun-set, which happened to be accurately observed by the famous mathematician *Geminiani Montanari*, as may be seen in his *Italian* treatise about it, published at *Bononia*: He observes, that at *Bononia* its greatest altitude in the S. S. E. was 38 degrees, and at *Siena* 58 degrees to the N. N. W. that its course, by the concurrent testimony of all observers, was from E. N. E. to W. S. W. that it came over the *Adriatic* sea, as from *Dalmatia*; that it crossed over all *Italy*, being nearly vertical to *Rimini* and *Savignano* on the one side, and to *Leghorn* on the other; that its perpendicular altitude was at least 38 miles; that in all places near this course, it was heard to make a hissing noise as it passed; that having past over *Leghorn*, it went off to sea towards *Corfica*; and lastly, that at *Leghorn* it was heard to give a considerable explosion, exceeding the report of a great gun, and that immediately after, there was heard another sort of sound, resembling the rattling of a great cart running over stones, which continued for some time: He concludes from its apparent velocity at *Bononia* at upwards of 50 miles distance, that it moved at about the rate of 160 miles in a minute of time, which is above 10 times as swift as the diurnal rotation of the earth under the equinoctial, and not many times less than that wherewith the annual motion of the earth about the sun is performed; to this *Montanari* adds its magnitude, which at *Bononia* appeared in one diameter bigger than the moon, and above half as big again in the other; which with the given distance of the eye, makes its real lesser diameter upwards of half a mile, and the other in proportion; this being supposed, it is not surprising, that so large a body, moving with such an incredible velocity thro' the air, tho' so much rarified as it is in its upper regions, should occasion so great a hissing noise, as to be heard at such a distance as it seems this was; but it will be much harder to conceive, how such an *Impetus* could be impressed on its body, which by several degrees exceeds that of any cannon-ball, and how this *Impetus* should be determined in a direction so nearly parallel to the horizon; and what sort of substance it must be, that could be so impelled, and ignited at the same time, there being no *Vulcano*, or other *Spiraculum* of subterraneous fire in the N. E. parts of the world, from whence it might be projected.

Dr. *Halley*, having much considered this appearance, takes it to be one of the hardest things to account for, that he had hitherto

therto met with in the *Phænomena* of meteors, and he was apt to think, that it must be some collection of matter formed in the *Æther*, by some fortuitous concurrence of atoms, as it were; and that the earth, met with it as it passed along in its orbit, being then but newly formed, and before it had acquired any considerable *Impetus* of descent towards the sun; for, its direction was exactly opposite to that of the earth, which formed an angle with the meridian at that time (the sun being in about 11 degrees of *Aries*) of 67 degrees, that is, its course was from W. S. W. to E. N. E. wherefore, the meteor seemed to move the contrary way; and besides falling into the power of the earth's gravity, and losing its motion from the opposition of the medium, it seems that it descended towards the earth, and was extinguished in the *Tyrrhene* sea to the W. S. W. of *Leghorn*; the explosion was heard upon its first immersion into the water; and the rattling like the driving a cart over stones was what succeeded upon its being extinguished; and something like this is always observed upon quenching a very hot iron in water: These facts being past dispute, the Dr. would be glad to have the opinion of the learned thereon, and to know what objection may reasonably be made against the abovesaid hypothesis.

There was much such another appearance seen at *Leipsick* in Germany in 1686, by M. *Gottfried Kirch*, who for several years was a very diligent observer of the heavens, and perfectly well acquainted with astronomical matters; in an *Appendix* to his *Ephemerides* for 1688, he gives the following remarkable account. 'On the 9th of July O. S. at an hour and a half in the morning, there was a glowing ball of fire with a tail observed in eight degrees and a half of *Aquarius*, and four degrees north, that continued immoveable for half a quarter of an hour, whose diameter was nearly equal to the semidiameter of the moon; at first its light was so considerable, that by means thereof we could read without a candle; it afterwards gradually vanished in its place: This *Phænomenon* was likewise observed at the same time in several other places, especially at *Schlaitz*, a town distant from *Leipsick* 11 German miles towards the south, being about 60 degrees above the southern horizon.' At the time of this appearance the sun was in 26 degrees and a half of *Capricorn*, and by the given place of the meteor, it is plain, it was seen about $\frac{1}{4}$ of an hour past the meridian, or in S. by W. and by its declination it could not be above 24 degrees high at *Leipsick*, tho' the same at *Schlaitz* was about 60 degrees high; the angle therefore, at the meteor, was

was about 36 degrees; whence by an easy calculation it will be found, that the same was not less than 16 German miles distant in a right line from *Leipsick*, and upwards of six and a half such miles perpendicular above the horizon, that is, at least 30 English miles high in the air; and tho' the observer says of it, *im-motus persistit per semiquadrantem horæ*, it is not to be understood, that it kept its place like a fixed star, all the time of its appearance, but that it had no very remarkable progressive motion; for, at the end of the said *Ephemerides* he has given a figure of it, which he has marked Fig. D, whereby it appears, that it darted obliquely downwards to the right hand, and where it ended, left two globules, or nodes, not visible but by a telescope.

The said M. *Gottfried Kirch* in the beginning of a treatise of his in *High Dutch*, concerning the great comet, which appeared in 1680, intitled *Aure Himmels Zeitung*, printed at *Nuremburg* 1681, gives an account of such another luminous meteor likewise seen at *Leipsick* on the 22d of May 1680. O. S. about three o'clock in the morning; which tho' he himself did not see, yet was observed there by divers persons who gave various accounts of it, but the more intelligent agreed, that it was seen descending in the north, and left behind it a long white streak, where it had passed; the like appearance was seen at the same time at *Haarburgh* in N. E. or rather N. N. E. as also at *Hamburg*, *Lubeck* and *Sralsund*, all which are about 40 German miles from *Leipsick*; but in all these places it was observed by persons unacquainted with the manner of describing properly things of this nature; so that all that can be concluded from it is, that this meteor was exceeding high above the earth, as well as the former: All the circumstances of these *Phænomena* agree with what was seen in *England* in 1708; but it commonly so happens, that these contingent appearances escape the eyes of those that are best qualified to give a good account of them; and when the like shall again happen, it were to be wished the curious would take more notice of them than has been hitherto done, that a better account may be given of such surprising appearances.

Remarks

Remarks on the Variations of the Magnetic Compass, published in the Memoirs of the Royal Academy of Sciences, with Regard to the general Chart of those Variations; as also concerning the true Longitude of the Magellan Straights; by Dr. Halley, Phil. Trans. N° 341. p. 165.

DR. Halley's chart published in 1701, for shewing at one view the variations of the magnetic compass, in all those seas with which the *English* navigators are acquainted, has been abundantly verified by the concurrent reports of the *French* pilots, who have had frequent opportunities of enquiring into the truth thereof; so that the Dr. hopes he has laid a sure foundation for the future discovery of an invention, that will be of considerable use to mankind, when perfected, he means that of the law, or rule whereby the said variations change, in appearance regularly, all over the world: Of this the Dr. has given his thoughts in *Phil. Trans.* N° 148, and 195, and hitherto he sees no cause to retract what he there offers for a reason of this change; but he thinks we might still arrive to a greater degree of certainty if we had a good collection of observations made in that ocean, which divides *Asia* and *America*, and which possesses about $\frac{2}{3}$ of the whole circumference of the globe.

Dr. Halley takes notice of two particulars, seeming to call in question the truth of his aforesaid map; the one is in the *Memoirs of the Royal Academy of Sciences* for 1700, concerning the variation observed at *Paraiba* in *Brasil*, about 25 leagues to the northward of *Pernambouc*, by M. Couplet the younger, whose words are as follows: 'On the 20th of May 1698, having first of all carefully traced a meridian line, of which I made use in my astronomical observations, I observed the declination of the needle to be $5^{\circ} 35'$ north-west.' And the same observer tells us, that he found the latitude of the town of *Paraiba* $6^{\circ} 38' 18''$: Now it happened, that the Dr. himself was in the river of *Paraiba*, in March 1699, and had full opportunity of observing the variation both on board his ship, and on shore, and found it constantly to be upwards of four degrees north-east; so that he is apt to take it an error of the press, the putting N. W. for N. E. or rather of M. Couplet's memory, who lost all his papers by shipwreck in his return: The like may be said of the latitude of *Paraiba*, which tho' the Dr. himself did not observe, yet at the fort of *Cabo Bello*, at the mouth of the river, and which is about three leagues more northerly than the town,

town, he found the latitude not less than $6^{\circ} 55'$ south, and consequently, that of the town more than 7° .

The other is in a discourse of M. de Lisle in the *Memoirs* of 1710, where he compared the variations observed in some later voyages with Dr. Halley's map of the variations; among other things it is there said, that on the east side of the island of St. Thomas under the equinoctial, M. Bigot de la Cansé, second lieutenant of the king's ship, *la Sphere*, had in the beginning of the year 1708 found the variation 11 degrees and half, whereas the Dr's chart makes it but 3 degrees and a half: The Dr. owns that he himself never observed in those parts, and that in such cases he was obliged to supply what was wanting from the accounts of others, and the analogy of the whole, and that it is possible, there might be more variation on that coast than he had allowed; but looking into his chart (which was fitted to the year 1700) the Dr. finds, he then makes the variation at the isle of St. Thomas fall 7 degrees and a half, and not 3 degrees and a half, which by the year 1708, might well arise to near 9 degrees; so that the difference will become pretty tolerable.

In the same *Memoir* of M. de Lisle, the geography of the Dr's chart is called in question; and we are told, that the Dr. has placed the entrance of the *Magellan* streights at least 10 degrees more westerly, than he ought to have done; for, that the ship *St. Louis* in 1708, sailing from the mouth of *Rio Gallega*, in about the latitude of 52 degrees south, and not far from *Cape Virgin*, directly for the *Cape of Good Hope* (which course, perhaps, was never run before) had found the distance between the two lands no more than 1350 leagues, which he concludes, is much less than the Dr's chart of the variation makes it: The Dr. does not know from what computation M. de Lisle has deduced this consequence, but he finds by his chart, that he has made the longitude of *Rio Gallega* 75 degrees west from *London*, and that of the *Cape of Good Hope* 16 degrees and a half east from it, that is in all, 91 degrees and a half difference of longitude; this with the two latitudes, gives the distance, according to the rhumb line, 1364 leagues, but according to the arch of a great circle, no more than 1287 leagues; so that instead of invalidating what the Dr. has there laid down, it absolutely confirms it, as far as the authority of one single ship's journals can do it.

The Dr. does not pretend, that he has had observations made with all the exactness requisite, for laying down incontestably the

the *Magellan* streights in their true geographical situation; yet he says it was not without good grounds he placed them as he had done; for when Sir *John Narborough* in 1670, wintered in Port St. *Julian* on the coast of *Patagonia*, Captain *John Wood*, then his lieutenant, and an approved artist in sea-affairs, observed the beginning of a lunar eclipse September 18. O. S. at just eight o'clock at night; and the same beginning was observed by *M. Hevelius* at *Dantzick* at 14 h. 22', whence Port St. *Julian* is more westerly than *Dantzick* 6 h. 22', or than *London* 5 h. 6', that is, 76 degrees and a half: Besides, the Dr. had had in his custody a very curious journal of one Captain *Strong*, who went into the *South Seas* in quest of a rich plate-wreck, and who discovered the two islands he called *Falkland's Isles*, lying about 120 leagues to the eastward of the *Patagon* coast, about the latitude of 51 degrees and a half: This Captain *Strong* had a quick passage from the island of *Trinidad* (in 20 degrees and a half south latitude) to the *Magellan* streights; and in this journal, which was very well kept, the Dr. found, that *Cape Virgin* was by his account 45 degrees of longitude more westerly than that island, whose longitude the Dr. knows to be just 50 degrees from *London*, that is, in all 75 degrees.

From these concurring testimonies, wanting better, the Dr. adventured to fix the longitude of this coast as he had done, and he can by no means grant an error of 10 degrees to be possible in it, tho' perhaps, it may need some smaller correction: The Dr. however, readily grants, that those, who go thither from *Europe*, shall find the land more easterly than is there expressed, by reason of a constant current setting to the westward near the *Equator*, where ships are many times long detained by calms, whilst the stream carries them along with it; this happens to all ships bound to any part of the east coast of *South America*.

Observations on the Comet in 1680. Phil. Trans. N° 342.
p. 170. *Translated from the Latin.*

THIS comet that appeared the latter end of 1680, was remarkable on several accounts; both for its being seen for four months, in which time it had run over nine entire signs, and for the extraordinary bigness and brightness of its tail; but especially for the remarkable curvity of its orbit, by means of which the theory of comets was at length discovered: for, whilst astronomers applied themselves with the greatest diligence to determine its motion from observations, the great Sir *Isaac Newton* was the first who demonstrated, that comets describe very nearly

parabolic orbits, which he has shewn how to construct from three given places, accurately observ'd, illustrating the subject by this comet of 1680, as may be seen at the latter end of the third book of his *Princip. Philosoph.*

But it happened, that this comet (which astronomers had so much observ'd in the evening) was not once observ'd in the morning, either at *Paris* or *Greenwich* before the 17th of *November*; whence it happened, that that part of the orbit, in which the comet descended to the sun, could not be determined with any certainty: *M. Gottfried Kirch* a *German*, publish'd at *Norimberg* in 1681, a book entitl'd *Neue himmels Zeitung*, i. e. *Novus Nuncius cœlestis*, where he shews, how he came to discover this comet, that had yet no tail, and that was scarcely discernible by the naked eye; to wit, whilst he was observing the moon and *Mars* that was near her, on the 4th of *November* O. S. in the morning, at *Colberg* in *Saxony*, a town 11 degrees more easterly than *London*, and about $50^{\circ} 20'$ latitude, the moon being now come to some star unknown to *Tycho*; (but which is in *Mr. Flamsteed's Catalogus Britannicus*, and the 44th star of *Leo*) he had a mind to determine the place of the said star from the fix'd stars near it; and whilst he was moving his telescope about, he lighted on a sort of nebulous spot, of an uncommon appearance, and which he presently concluded, was either a new comet, or a nebulous star, resembling that in the girdle of *Andromeda*: He first saw this comet half an hour after 4 o'clock in the morning, somewhat higher than the two small telescopic stars, represented by *a* and *c*, Plate IV. Fig. 3. with which at 6 o'clock it was seen exactly in a streight line; whence it appeared that it moved, and that direct; between 5 and 6 o'clock he viewed this phenomenon with his 10 foot telescope, and saw two other small stars near it, less than the former, represented by *e* and *d*, and above these a third star *g*; the distance of the comet was somewhat less from *e* than from *a*, but the distance *de* was greater; at $6^h 38'$ the distance of the comet from *e*, was double of that between *d* and *e*, and the line *de* being produced, left the comet below it, the telescope inverting the objects, yet so as to touch its superior margin; at $6^h 45'$ the comet was more sensibly distant from *e* than from *a*, and from *a* a little more than half the distance of the small stars *a* and *g*: It is to be observ'd, that the clock had anticipated the heavens by 14 entire minutes, as appear'd from the altitudes of *Cor Leonis* taken at that time.

This is a very noble observation, and therefore several methods were used to find out the places of the small stars then near the comet, and it appeared, that at that time they had the following situation.

		Long.			Lat.			
a	♈	29°	54'	20"	1°	29'	20"	North
d		29	27	20	1	8	00	
e		29	34	30	1	10	45	

A great circle, drawn thro' *a* and *e*, was found to pass thro' the last star of the tail of *Ursa major*, and consequently, the angle form'd at *a* with the circle of longitude was $15^{\circ} 36'$ and $\frac{1}{2}$; and since the distance of the comet from *a* towards *e* was a little more than half the distance *ag* (which was found by a 16 foot telescope and a micrometer to be 21 minutes and $\frac{1}{2}$) let us suppose, that it was 12 minutes, and from these data the place of the comet will be found to be $\delta 29^{\circ} 51'$ with $1^{\circ} 17' \frac{1}{2}$ N. Lat. at 6 o'clock, but at London $5^h 2'$ of apparent time,

Afterwards on the 6th of November at $4^h 42'$ in the morning, M. Kirch did with his two foot telescope, observe the comet exactly in a streight line between *Mars* and the small star N, which is the 45th of *Leo* in the *Catalogus Britannicus*, and then it was in $\pi 2^{\circ} 42'$ with $0^{\circ} 16' \frac{1}{2}$ S. Lat. *Mars* had at that time (upon comparing together the observations made just before and after) $\pi 3^{\circ} 46' \frac{1}{2}$ with $1^{\circ} 56'$ N. Lat. whence from its given path the comet's place, at London $3^h 58'$ in the morning, apparent time, was $\pi 3^{\circ} 23'$ with $1^{\circ} 6'$ N. Lat. November the 11th at $5^h 13'$ in the morning the comet was equally distant from *Dayer's* two stars of *Leo* σ and τ , but had not yet reached the right line that joins them, tho' very near it: In the *Catalogus Britannicus* σ had at that time $\pi 14^{\circ} 15'$ and almost $1^{\circ} 41'$ N. Lat. but τ had $\pi 17^{\circ} 3' \frac{1}{2}$ and $0^{\circ} 34'$ S. Lat. consequently, the Lat. of the comet was somewhat less than the mean between these, viz. than $0^{\circ} 33' \frac{1}{2}$ north, and its Long. somewhat less than $\pi 15^{\circ} 39'$; but this is not to be relied on, seeing it depends upon the estimated equality of the distances, which is uncertain; the tail of the comet now began to be only half a degree in length, seen thro' a ten foot telescope.

An Account of the Dispute between M. Leibnitz and Mr. Keill, about the Right of the Invention of the Method of Fluxions, by some call'd the Differential Method. Phil. Transf. N^o 342, p. 173.

Several accounts having been publish'd abroad of the *Commercium epistolicum Collinii & aliorum de Analyfi promotâ*, and all of them very imperfect, it was thought proper to publish the following account.

This *Commercium* consists of several letters and papers, in the custody of the *Royal Society*, and which are here ranged together in order of time, and either faithfully copied, or translated into *Latin* from such originals as are mentioned in the title of each letter and paper; and a numerous committee of the *Royal Society* was appointed to examine the sincerity of the originals, and compare therewith the copies taken from them: It relates to a general method of resolving finite equations into infinite ones, and applying these equations, both finite and infinite, to the solution of problems, by the method of fluxions and moments: We will first give an account of that part of the method, which consists in resolving finite equations into infinite ones, and by that means squaring curvilinear figures; by infinite equations are meant such as involve a series of terms converging, or approaching still nearer and nearer to the truth in *infinitum*, so as at length to differ therefrom by a quantity less than any given, and if continued in *infinitum* to leave no difference.

Dr. Wallis in his *Opus Arithmeticum* publish'd in 1657 Cap. 33. Prop. 68. reduced the fraction $\frac{A}{1-R}$ by a continual division

into the series $A + AR + AR^2 + AR^3 + AR^4 + \text{Ec.}$ Lord Viscount Brouncker squared the hyperbola by this series

$\frac{1}{1 \times 2} + \frac{1}{3 \times 4} + \frac{1}{5 \times 6} + \frac{1}{7 \times 8} + \text{Ec.}$ that is, by this $1 - \frac{1}{2} + \frac{1}{2} - \frac{1}{4} + \frac{1}{4} - \frac{1}{6} + \frac{1}{6} - \frac{1}{8} + \text{Ec.}$ conjoining each two terms into one; which was publish'd in the *Philosophical Transactions* for April 1668.

Soon after, M. Mercator publish'd a demonstration of this quadrature by Dr. Wallis's division; and not long after that, Mr. James Gregory gave a geometrical demonstration thereof; and these books were a few months after sent by Mr. John Collins to Dr. Barrow at Cambridge, and by Dr. Barrow communicated to Mr. Newton in June 1669, then a fellow of Trinity College;

College; whereupon Dr. Barrow lent Mr. Collins in return a tract of Mr. Newton's, entitled *Analystis per equationes numero terminorum infinitas*; and this is the first piece publish'd in the *Commercium*, and contains a general method of doing that in all curves, which my Lord Brounker, and M. Mercator did in the hyperbola alone; and tho' the latter liv'd upwards of 10 years after, yet he never proceeded any farther than to the single quadrature of the hyperbola: The progress made by Mr. Newton as to all curves in general shews, that he did not want M. Mercator's assistance; however, to avoid cavilling, he allows that both my Lord Brounker had invented, and M. Mercator demonstrated the series for the hyperbola some years before they publish'd it, and consequently before he himself had found out his general method.

Mr. Newton in his letter to Mr. Oldenburgh, dated October 24. 1676, mentions the aforesaid treatise of *Analystis* in the following manner; 'at that very time in which Mercator's *Logarithmotechnia* was publish'd, my friend Dr. Barrow (then professor of mathematics at Cambridge) communicated to Mr. Collins a *Compendium* of these series, wherein I signified, that the area's of all sorts of curves and their rectification, together with the superficies and contents of solids could be determined from given right lines, and vice versa; and I illustrated the said method by divers series': Of this *Compendium* Mr. Collins in several letters in 1669, 1670, 71, and 72 informed Mr. James Gregory in Scotland, Mr. Bertet and Mr. Vernon at Paris, S. Alphonso Borelli in Italy, and Mr. Strobe, Mr. Townsend, Mr. Oldenburgh, Mr. Dary and others in England, as still appears by his letters; and besides, Mr. Oldenburgh in a letter dated September 14. 1669, and enter'd in the letter-book of the Royal Society, communicated it to M. Francis Slusius at Leige, and cited several passages out of it; and particularly Mr. Collins in a letter to Mr. James Gregory, dated November 25. 1669, speaks thus of the method contained therein; 'Dr. Barrow has given up his place of reading public lectures to one Mr. Newton of Cambridge, whom he mentions in the preface to his *Optical Lectures*, as a person of an extraordinary genius; for, before Mercator's *Logarithmotechnia* was publish'd, he had invented the same method, and applied it to all curves in general, and to the circle divers ways'. And in a letter to Mr. David Gregory, dated August 11. 1676, he speaks of it to the following purpose, 'a few months after these books (*viz.* Mercator's *Logarithmotechnia*, and Gregory's *Exercitationes geometricæ*) were publish'd, they were sent to

Dr.

Dr. Barrow at Cambridge; and the Dr. returned answer, that this doctrine of infinite series was invented by Mr. Newton two years before Mr. Mercator's *Logarithmorechnia* was publish'd, and that he had applied it to all curves in general, and the Dr. at the same time sent Mr. Newton's MS. copy. The last of the said two books came out towards the latter end of 1668, and Dr. Barrow sent the said *Compendium* to Mr. Collins in July following, as appears by three of the Dr.'s letters; and Mr. Collins in a letter to Mr. Strode, dated July 26. 1672, writes to the following purpose; 'I sent a copy of the *Logarithmorechnia* to Dr. Barrow at Cambridge, who immediately sent me some papers of Mr. Newton's, from which, and others formerly communicated to the Dr. by the author, it appears that that method was invented some years before by the said Mr. Newton, and applied universally; so that by means thereof, in any propos'd curvilinear figure, defin'd by one or more properties, the quadrature or area of the said curve may be found, accurately, if possible, but if not, yet infinitely near; as also the evolution or length of a curve, the center of gravity of a figure, solids generated by its rotation, and their superficies may be obtained without any extraction of roots: After that Mr. Gregory understood, that this method, us'd by Mr. Mercator in his *Logarithmorechnia*, and applied to the quadrature of the hyperbola, and likewise improv'd by the said Mr. Gregory himself, was now made universal, and applied to all sorts of figures, he after a great deal of close application discover'd the same method; and both Mr. Newton, and Mr. Gregory propose to improve it; but the latter does not think it fair to anticipate Mr. Newton, the first inventor': And in another letter to Mr. Oldenburgh, to be communicated to Mr. Leibnitz, and dated June 14. 1676, Mr. Collins adds; 'such is the excellency of this method, that being so universal it sticks at no difficulty; and I believe, that Mr. Gregory and others are of opinion, that whatever was known before of this matter was but like the glimmering light of the dawn, when compar'd with the brightness of noon-day.'

This tract was first published by Mr. William Jones in 1710, who found it among the papers, and in the hand writing of Mr. John Collins, and afterwards collated it with the original which he borrowed of Mr. Newton; it contains the above-mentioned general method of *Analysis*, shewing how to resolve finite equations into infinite ones, and how by the method of moments to apply equations both finite, and infinite to the solution

tion of all sorts of problems; it begins where Dr. Wallis left off, and founds the method of quadratures upon three rules.

Dr. Wallis published his *Arithmetica infinitorum* in 1655, and by the 59th Prop. of that book, if the *Abscissa* of any curvilinear figure be called x , and m and n be integers, and the ordinates erected at right angles be $x^{\frac{m}{n}}$, the area of the figure

will be $\frac{n}{m+1} x^{\frac{m+1}{n}}$. And this is assumed by Mr. Newton

as the first rule, upon which he founds his quadrature of curves: Dr. Wallis demonstrated this proposition by an induction of particulars, and then collected all into one proposition by a table of the cases; whereas Mr. Newton reduced all the cases into one by a power with an indefinite index, and at the end of his *Compendium* demonstrated it at once by his method of moments, and he was the first who introduced indefinite indices of powers into analytical operations: Besides, by the 108th proposition of the said *Arithmetica infinitorum*, and by several other propositions that follow; if the ordinate be composed of two or more ordinates, taken with their signs $+$ and $-$, the area will be composed of two or more areas, taken with their signs $+$ and $-$ respectively; and this is assumed by Mr. Newton as the second rule, upon which he founds his method of quadratures: And the third rule is to reduce fractions, and radicals, and the affected roots of equations into converging series, when the quadrature does not otherways succeed; and by the first and second rules to square the figures, whose ordinates are the single terms of the Series: Mr. Newton in a letter to Mr. Oldenburgh, dated June 13, 1676, and communicated to M. Leibnitz, shewed how to reduce any power of any binomial into a converging Series, and how by that Series to square the curve, whose ordinate is that power; and being desired by M. Leibnitz to explain the origin of this theorem, he replied in his letter, dated Oct. 24, 1676, that a little before the plague (which raged in London in 1665) upon reading Dr. Wallis's *Arithmetica infinitorum*, and considering how to interpolate the series $x, x - \frac{1}{2}x^2, x - \frac{1}{2}x^2 + \frac{1}{2}x^4, x - \frac{1}{2}x^2 + \frac{1}{2}x^4 - \frac{1}{2}x^6, \&c.$ he found the area of a circle to be $x - \frac{1}{3}x^3 + \frac{1}{5}x^5 - \frac{1}{7}x^7 + \frac{1}{9}x^9 - \&c.$ and by pursuing the method of interpolation he found the theorem above-mentioned, and by means of this theorem, the reduction of fractions and surds into converging series, by division and extraction of roots, and that then he proceeded to the extraction of affected roots; and these reductions are his third rule.

When

When Mr. *Newton* had in this *compendium* explained these three rules, and illustrated them with variety of examples, he laid down the idea of deducing the area from the ordinate, by considering the area as a nascent quantity, growing or increasing, by continual flux, in proportion to the length of the ordinate; and supposing the *Abscissa* to increase uniformly in proportion to the time; and from the moments of the time he gave the name of moments to the momentaneous increments, or infinitely small parts of the *Abscissa* and area, generated in moments of time; the moment of a line he called a point, in the sense of *Cavallerius*, tho' it be not a geometrical point, but a line infinitely short; and the moment of an area, or superficies he called a line in the sense of the same *Cavallerius*, tho' it be not a geometrical line, but a superficies infinitely narrow; and when he considered the ordinate, as the moment of the area, he understood by it the rectangles under the geometrical ordinate, and a moment of the *Abscissa*, tho' that moment be not always expressed: 'Let *A B D* Plate IV. Fig. 4. says he, be any curve, and *A H K B* a rectangle, whose side *A H* or *K B* is unity; and suppose, the right line *D B K*, moving uniformly from *A H*, to describe the areas *A B D* and *A K*, and that the right line *B K*, x , is the moment by which the area *A K*, x , gradually increases, and the right line *B D*, y , the moment by which the curvilinear area *A B D* gradually increases; and that from the moment *B D* continually given, you may by the three preceeding rules investigate the area *A B D*, described thereby, or compare it with the area *A K*, x , described by the moment x .'

This is Mr. *Newton's* idea of squaring curves; and how he applies this to other problems, he expresses in the next words; 'now, says he, by the same method that the superficies *A B D* is found by the three preceeding rules from its moment being continually given, by the very same any other quantity may be found from its moment given in like manner; the matter will become plainer by an example;' and after some examples he adds his method of regression from the area, arch, or solid contents, to the *Abscissa*; and shews how the same method extends to mechanical curves, for determining their ordinates, tangents, area's, lengths, &c. and that by assuming any equation, expressing the relation between the area and *Abscissa* of a curve, you may by this method find the ordinate; and this is the foundation of the method of fluxions, and moments, which Mr. *Newton* in his letter, dated *October 24, 1676*, comprehended in the following

ing problem; from a given equation involving any number of fluents, or flowing quantities, to find the fluxions, and *Vice Versa*.

In this *Compendium* Mr. Newton represents the uniform fluxion of time, or of any exponent of time by an unit; the moment of time, or of its exponent by the letter o ; the fluxions of other quantities by any other symbols; the moments of those quantities by the rectangles under those symbols, and the letter o ; and the area of a curve by the ordinate inclosed in a square, the area being put for a fluent, and the ordinate for its fluxion; when he is demonstrating any proposition, he uses the letter o for a finite moment of time, or of its exponent, or of any quantity flowing uniformly, and performs the whole calculation by the geometry of the ancients in finite figures, or schemes, without any approximation; and as soon as the calculation is at an end, and the equation reduced, he supposes the moment o to decrease in infinitum and vanish; but when he is not demonstrating, but only investigating a proposition, for making dispatch, he supposes the moment o to be infinitely small, and forbears to write it down, and uses all manner of approximations, which he conceives will produce no error in the conclusion: You have an example of the first kind at the latter end of this *Compendium*, in demonstrating the first of the three rules, laid down in the beginning of the book; you have examples of the second kind in the same *Compendium*, p. 15. in finding the length of curve lines; and in p. 18, 19, in finding the ordinates, area's, and lengths of mechanical curves; and he tells you p. 19, how by the same method to draw tangents to mechanical curves; and in his letter of December 10, 1672 he adds, that problems about the curvature of geometrical, or mechanical curves are resolved by the same method; whence it is manifest, that he had then extended the method to second and third moments; for when the area's of curves are considered as fluents (as is usual in this *Analysis*) the ordinates express the first fluxions, and the tangents are given by second fluxions, and the curvatures by third; and even in this *Analysis* p. 16. where Mr. Newton says, *Momentum est superficies cum de solidis, & linea cum de superficiebus & punctum cum de lineis agitur*; is the same as if he had said, that when solids are considered as fluents, their moments are superficies, and the moments of those moments, or second moments, are lines, and the moments of those moments, or third moments are points in the sense of Cavallerius; and in his *Princip. Philosoph. Mathematic.* where he frequently considers lines as fluents, described by points

whole velocities increase, or decrease, the velocities are the first fluxions, and their increase the second; and the problem, *viz.* from a given equation involving flowing quantities to find the fluxions, and *Vice versa*, extends to all the fluxions, as is manifest by the examples of its solution, published by Dr. Wallis *Tom. II. p. 391, 392, 396*, and in *lib. 2. Princip. prop. 14*. Mr. Newton calls the second difference, the difference of moments.

Now the better to understand what kind of calculation Mr. Newton used in, or before 1669, when he wrote this *Compendium* of his *Analysis*, his demonstration of the first rule above-mentioned is as follows; 'let the basis $Fi. 3.$ AB of any curve $AD\delta$ be $= x$, and the ordinate $BD = y$, and the area $ABD = z$, 'as before; likewise let $B\beta = o$, $BK = v$, and the rectangle $B\beta HK$ (ov) $=$ the space $B\beta\delta D$; therefore, $A\beta$ will be $= x + o$, and $A\delta B = z + ov$; these things being premised, from assuming at pleasure the relation between x and z , 'y is sought for as follows; let the equation $\frac{2}{3}x^{\frac{3}{2}} = z$, or $\frac{2}{3}x^3 = z^2$ be taken at pleasure; then substituting $x + o$ or $A\beta$ for x , and $z + ov$, or $A\delta B$ for z , you have $\frac{2}{3}$ into $x^3 + 3x^2o + 3xo^2 + o^3 =$ (from the nature of the curve) $z^2 + 2zov + o^2v^2$, and taking away equals, *viz.* $\frac{2}{3}x^3$ and z^2 , and dividing the rest by o , there remains $\frac{2}{3}$ into $3x^2 + 3xo + o^2 = 2zv + ov^2$; now if we suppose, that $B\beta$ decreases in infinitum and vanishes, or that o is nothing, v and y will be equal, and the terms multiplied by o will vanish; 'consequently, there will remain $\frac{4}{9} \times 3xx = 2zv$, or $\frac{2}{3}xx$ $(= zy) = \frac{2}{3}x^{\frac{1}{2}}y$, or $x^{\frac{1}{2}} (= \frac{x^2}{x^{\frac{1}{2}}}) = y$; wherefore *è contra* if $x^{\frac{4}{3}} = y$, $\frac{2}{3}x^{\frac{1}{3}}$ will be $= x$: Or universally, if $\frac{n}{m+n} \times ax^{\frac{m+n}{n}} = \frac{m+n}{n} = x$; or putting $\frac{na}{m+n} = c$, and $m+n = p$; if $c x^{\frac{p}{n}} = x$, or $c^n x^p = x^n$, then putting $x + o$ for x , and $z + ov$ or (which is the same thing) substituting $x + oy$ for z , you have $c^n \times x^p + po x^{p-1} \&c. = x^n + noy x^{n-1} \&c.$ 'to wit, by omitting the other terms of the *Series*, that at length would vanish; now taking away the equal terms $c^n x^p$ and x^n , and dividing the rest by o , there remains $c^n p x^{p-1} = ny x^{n-1}$ $(=$

$(= \frac{nyx^n}{x} = \frac{nyc^n x^p}{c^n n})$ or dividing by $c^n x^p$, $p x - 1$ will be

$= \frac{ny}{c^n n}$ or $p c^n = \frac{p-n}{n} = ny$, or by restoring again $\frac{na}{m+n}$,

instead of c , and $m+n$ instead of p , that is, m instead of

$p-n$, and xa instead of pc , $ax^{\frac{m}{n}}$ will be $=y$; wherefore

et contra, if $ax^{\frac{m}{n}}$ be $=y$, $\frac{n}{m+n} ax^{\frac{m+n}{n}}$ will be $=x$

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The second rule may also be demonstrated by the same method of operation; and if any equation whatever be assumed, expressing the ratio between the *Abscissa* and area of a curve, the ordinate may be found in the same manner, as is hinted in the next words of the *Analysis*; and if this ordinate multiplied into unity be put for the area of a new curve, the ordinate of this new curve may be found in the same manner; and so on perpetually; and these ordinates represent the 1st, 2d, 3d, 4th, and following fluxions of the first area: This was Mr. Newton's method of operation at the time, when he wrote this *Compendium* of his *Analysis*; and he used the same method in his book of quadratures, and still continued to use it: Among the other examples, whereby he illustrates the method of *Series* and moments set down in this *Compendium*, are these; let the radius of a circle be 1, the arch x , and the sine x ; the equations for finding the arch, whose sine is given, and finding the sine whose arch is given

$$\begin{cases} x = x + \frac{1}{6} x^3 + \frac{3}{40} x^5 + \frac{5}{112} x^7 + \frac{35}{1152} x^9 + \text{Ec.} \\ x = x - \frac{1}{6} x^3 + \frac{1}{120} x^5 - \frac{1}{5040} x^7 + \frac{1}{362880} x^9 - \text{Ec.} \end{cases}$$

Mr. Collins informed Mr. Gregory of this method in autumn 1669, and Mr. Gregory by the help of one of Mr. Newton's *Series*, after a year's study, found out the method in December,

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from the first Series take all the terms but the two first,

there will remain $\frac{2}{3} = \frac{1}{2 \times 3} + \frac{1}{2 \times 4} + \frac{1}{3 \times 5} + \frac{1}{4 \times 6} + \dots$ &c.

About the latter end of February or beginning of March 1673, M. Leibnitz went from London to Paris, and continued his correspondence with Mr. Oldenburgh to the following June; then he learned Algebra and the higher Geometry, and in July 1674 he renewed his correspondence with Mr. Oldenburgh and Mr. Collins, and he wrote, that he was master of a wonderful theorem, which gave accurately the Area of a circle, or any Sector thereof, in a series of rational numbers; and in October following, that he had found the circumference of a circle, in a series of very simple numbers, and that by the same method (so he calls the said theorem) any arch, whose sine was given, might be found in a like series, tho' the ratio to the whole circumference were not known; his theorem, therefore, amounted to the finding any Sector or arch, whose sine was given; if the ratio of the arch to the whole circumference was not known, the theorem or method gave him only the arch; if it was known it also gave him the whole circumference; and therefore it was the first of Mr. Newton's two theorems above-mentioned; but M. Leibnitz did not then know the demonstration of this theorem; for, in his letter of May 12, 1675, he desir'd Mr. Oldenburgh to procure the demonstration from Mr. Collins, meaning the method by which Mr. Newton had invented it.

In a letter of Mr. Collins's, dated April 15, 1675, Mr. Oldenburgh sent M. Leibnitz eight of Mr. Newton's and Mr. Gregory's Series, amongst which were Mr. Newton's two Series above-mentioned, for finding the arch, whose sine is given, and the sine, whose arch is given; and Mr. Gregory's two Series above-mentioned, for finding the arch, whose tangent is given, and the tangent, whose arch is given; and M. Leibnitz in his answer dated May 20, 1675, acknowledged the receipt of this letter in the following words, 'I receiv'd your letter containing a great deal of algebraical knowledge, for which I thank you and the learned Mr. Collins; but being now very much taken up; besides my ordinary business, especially with mechanical affairs, I could not examine the series you sent me, and compare them with my own; as soon as I shall have done it, I shall give you my opinion; for, it is some years ago since I invented my series in some very peculiar way'.

But

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at yet Mr. Leibnitz never after took any farther notice of his
 ing receiv'd these series, nor hinted how his own differ'd from
 nem, nor ever produced any other series than those he re-
 ceiv'd from Mr. Oldenburgh, or numeral series deduced from
 them in particular cases; and what he did with Mr. Gregory's
 series for finding the arch, whose tangent is given, he has told us
 in the *Acta Eruditorum* for the month of April, 1691, p. 178,
 'now, says he, in 1675, I had compos'd a small treatise of arith-
 metical quadrature, that from that time was read by my friends
 ' &c'. By a theorem for the transmutation of figures, like those of
 Dr. Barrow and Mr. Gregory, he had now found a demonstra-
 tion of this series, and this was the subject of his *Opusculum*, or
 treatise; but he still wanted a demonstration of the rest, and for
 this purpose he wrote, to Mr. Oldenburgh the following letter
 dated at Paris May 12. 1676. 'when M. George Mohr a Dane
 ' brought me the method of expressing the ratio between the arch
 ' and the sine by the following infinite series, that had been com-
 ' municated to him by your learned Mr. Collins; viz. putting x
 ' for the sine, z for the arch, and 1 for the Radius $x = x +$

$$+ \frac{1}{2} x^3 + \frac{3}{40} x^5 + \frac{5}{112} x^7 + \frac{35}{1152} x^9 + \text{&c}.$$

$$x = z - \frac{1}{2} z^3 + \frac{1}{120} z^5 - \frac{1}{5040} z^7 + \frac{1}{362880} z^9 - \text{&c}$$

' when, I say, he brought me these, which appear to me to be
 ' very ingenious, and especially the latter series, which has a
 ' certain peculiar elegance; therefore, Sir, you shall oblige me
 ' very much, if you send me the demonstration; in return I
 ' shall send you my thoughts on this matter, which are very
 ' different from these, and of which I think I have writ you
 ' about some years ago, without adding the demonstration,
 ' which I am now about polishing; I intreat you would remem-
 ' ber me very kindly to Mr. Collins, who can enable you to
 ' satisfy my desire'; here, by the expression, I say, one would
 think, he had never seen these two series before, and that his
 thoughts on this matter, which are very different from these,
 were something other than one of the series, which he had re-
 ceived from Mr. Oldenburgh the year before, and that a demon-
 stration thereof which he was then polishing, would be of such a
 value, as to compensate for Mr. Newton's method.

Upon the receipt of this letter, Mr. Oldenburgh, and Mr.
 Collins wrote pressingly to Mr. Newton, desiring he would de-
 scribe his method to be communicated to Mr. Leibnitz; upon
 this

this Mr. Newton wrote his letter, dated *June 13, 1676*, describing therein his method of series, as he had done before in the *Compendium* above-mentioned; only with this difference that here he describ'd at large the reduction of the power of a binomial into a series, and only touch'd upon the reduction by division, and extraction of affected roots, because in the *Compendium* he had already described these latter; but he only set down the two first terms of the Series, into which the power of a binomial might be reduced; and among the examples in this letter, there were Series for finding the number whose logarithm is given, and for finding the versed sine whose arch is given; this letter was sent to *Paris June 26, 1676*, together with a MS. drawn up by Mr. Collins, containing extracts of Mr. James Gregory's letters; for, Mr. Gregory died about the end of 1675; and Mr. Collins, at the request of M. Leibnitz and some others of the *Academy of Sciences*, drew up extracts of his letters, and the collection is still extant in the hand-writing of Mr. Collins, entitled, *Extracts of Mr. Gregory's letters, to be lent to M. Leibnitz to peruse, who is desired to return the same to you, viz. Mr. Oldenburgh*; and that they were sent is affirmed by Mr. Collins in his letter to Mr. David Gregory the brother of the deceased, dated *August 11, 1676*, and appears farther by M. Leibnitz and M. Tschirnhauser's answers concerning them.

M. Leibnitz's answer, directed to Mr. Oldenburgh, and dated *August 27, 1676*, begins thus, 'your letter dated *July 26*, contains several, and those more extraordinary things in *Analysis*, than a great many voluminous books published on this subject; wherefore, I thank both you, Mr. Newton, and Mr. Collins for communicating to me so many curious things'; and towards the end of the letter, after he had done with the contents of Mr. Newton's letter, he proceeds thus; 'I come now to other things contained in your letter, which the learned Mr. Collins was pleas'd to communicate; I wish he had added the demonstration of Mr. Gregory's linear approximation, for he certainly had a genius for promoting such speculations'; And M. Tschirnhauser's answer, dated *September 1, 1676*, after he had done with Mr. Newton's letter about series, concludes thus, 'and what that excellent geometrician Mr. Gregory has done in this matter are certainly extraordinary; and indeed such as shall cause his MSS to be published, shall do the greatest service to his reputation.' In the first part of this letter, where M. Tschirnhauser speaks of Mr. Newton's series, he says, he looked over them cursorily, to see if he could find M. Leibnitz's series for

for squaring the circle, or hyperbola; but if he had search'd for it in the extracts of Mr. Gregory's letters, he might have found it in the above mentioned letter of February 15. 1671; for, the MS. of these extracts, with that letter therein, is still extant in the hand-writing of Mr. Collins. And tho' M. Leibnitz had now received this series twice from Mr. Oldenburgh, yet in his letter of August 27. 1676, he sent it back to him by way of recompence for Mr. Newton's method, pretending, he had communicated it to his friends at Paris three years before, and upwards; that is, two years before he received it in Mr. Oldenburgh's letter of April 15. 1673; at which time he did not know it to be his own, as appears by his answer of May 20. 1673, abovementioned. He might receive this series at London, and communicate it to his friends at Paris above three years before he sent it back to Mr. Oldenburgh; but it does not appear, that he had the demonstration thereof so early; which when he had found out, he then exhibited in his *Opusculum*, and communicated it to his friends, and he himself has told us, that this was in 1673. However, it lies upon him to prove, that he had this series, before he receiv'd it from Mr. Oldenburgh; for, in his answer to Mr. Oldenburgh he did not know any of the series, then sent him, to be his own; and he conceal'd from the gentlemen at Paris his having receiv'd it from Mr. Oldenburgh, with several other series, and his having seen a copy of the letter, in which Mr. Gregory had sent it to Mr. Collins in the beginning of the year 1671. In the same letter of August 27. 1676, after M. Leibnitz had describ'd his quadrature of the circle, and equilateral hyperbola, he adds; again from the series of regressions I found the following for the hyperbola, viz: if any number be less than unity, as $1 - m$, and its hyperbolic logarithm be l ; m will be $\frac{l}{1} - \frac{l^2}{1 \times 2}$

$+ \frac{l^3}{1 \times 2 \times 3} - \frac{l^4}{1 \times 2 \times 3 \times 4} + \text{Ec.}$ if the number be greater

than unity, as $1 + n$, then for finding it, I have likewise discovered the rule, express'd in Mr. Newton's letter; viz. n

will be $= \frac{l}{1} + \frac{l^2}{1 \times 2} + \frac{l^3}{1 \times 2 \times 3} + \frac{l^4}{1 \times 2 \times 3 \times 4} + \text{Ec.}$

— and as to the regression from arches, I directly lighted upon the rule that from the given arch gives the co-sine; viz.

the co-sine $= 1 - \frac{a^2}{1 \times 2} + \frac{a^4}{1 \times 2 \times 3 \times 4} - \text{Ec.}$ but afterwards

I like.

I likewise found, that from this rule might be demonstrated that other communicated to me for finding the right sine which is $\frac{a}{1} - \frac{a^3}{1 \times 2 \times 3} + \frac{a^5}{1 \times 2 \times 3 \times 4 \times 5} - \dots$ &c. Thus M.

Leibnitz put in his claim for the co-invention of these four series, tho' the method of finding them was sent him at his own request, which yet he could not understand; for, in the same letter of *August 27. 1676*, he desired Mr. *Newton* to explain it farther, his words are as follows; 'but I want Mr. *Newton* should explain some things farther; as the origin of the theorem he first lays down; the method, by which in his operations, he found the quantities *p, q, r*; and in fine, how he proceeds in the method of regressions, as when from the logarithm the number is sought; for, he does not explain how that is deduced from his method'.

He pretended to have found two series for the number whose logarithm was given, and yet in the same letter he desired Mr. *Newton* to explain the method of finding those very two series.

When Mr. *Newton* receiv'd this letter, he wrote back, that he had already communicated to him all the said four series, the two former being one and the same series, putting the letter *l* for the logarithm with its sign $+$ or $-$; and the third the excess of the *Radius* above the versed sine, for which a series had been formerly sent him; upon this, M. *Leibnitz* desisted from his claim. Besides, Mr. *Newton* in the same letter, dated *October 24. 1676*, farther explained his methods of regression, as M. *Leibnitz* had desired; and yet M. *Leibnitz* in his letter of *June 21. 1677*, desired a farther explication; but soon after, upon reading Mr. *Newton's* letter a second time, he wrote back *July 12. 1677*, that he now understood what he wanted, and found by his papers, that he had formerly us'd one of Mr. *Newton's* methods of regression; but as in the example which he then chanced to use, there was nothing elegant, he had, out of his usual impatience, neglected to use it any farther; He had, therefore, several direct series, and, consequently, a method of finding them, before he invented the inverse method, but afterwards forgot it; and if he had search'd his old papers more diligently, he thinks he might also have found this method there, but having forgot his own methods, he wanted Mr. *Newton's*.

When Mr. *Newton* in his letter dated *June 13. 1676*, had explained his method of series, he added; 'from these things it appears, how much the bounds of *Analysis* are enlarged by

such sort of infinite equations; for, by means thereof, it extends itself, I had almost said, to all problems, excepting the numerical ones of *Diophantus*, and the like; yet it does not become altogether universal, but by some other methods of finding out infinite series; for, there are some problems where we cannot come to infinite series either by division, or extraction of simple or affected roots; but how we are to proceed in these cases, I have not leisure now to shew, or mention some other things I have invented about the reduction of infinite series into finite ones, where the nature of the thing will bear it; for, I forbore writing on these speculations, now for almost these five years past, because I have long since been tir'd of them.' To this *M. Leibnitz* in his letter of August 27. 1676 answer'd; 'what you seem to say, that almost all difficulties (excepting *Diophantus*'s problems) may be reduced to infinite series, I cannot come in to; for, there are several problems so intricate and perplex'd, as not to depend either on equations, or quadratures; such are amongst a great many others, the problems of the inverse method of tangents'. And *Mr. Newton* in his letter of October 24. 1676, replied; 'when I said, that almost all problems might be solv'd, I would be understood to mean those especially, about which mathematicians have already employ'd themselves, or at least such as can admit of mathematical reasoning; for, it is true, others may be devised so involved with perplex'd conditions, that we cannot sufficiently comprehend them, and much less bear the fatigue of such prodigious calculations, as they may require: Yet notwithstanding, that I may not seem to exceed the bounds of modesty, I can resolve both the inverse problems of tangents, and others more difficult; and for this purpose I make use of a twofold method, the one more concise, and the other more general; at present I thought proper to express both in transposed letters, that I might not be obliged, on account of others, finding out the same thing, to alter my design in some respects; $saccde$ to $effb$, &c. that is, one method consists in taking away the fluent, or flowing quantity from an equation, that at the same time involves its fluxion; the other method, only in assuming a series for any unknown quantity, from which the rest may be commodiously deduced; and in comparing the homologous terms of the equation resulting to find out the terms of the assumed series'. By these two letters of *Mr. Newton*'s, it is certain, that he had then, or rather upwards of five years before, found out the reduction of problems to fluxional equations, and converging series; and by *M. Leibnitz*'s answer

answer to the first of these letters, it is ascertain, that the latter had not then found out the reduction of problems, either to differential equations, or to converging series; and the same is also manifest by what M. *Leibnitz* wrote in the *Acta Erudit.* Anno. 1691, concerning this matter: 'Now in 1675, says he, I had compos'd a small treatise of arithmetical quadrature, which from that time had been perus'd by my friends, but, the matter growing upon my hands, I had not leisure to prepare it for the press, after that, other avocations interfer'd; especially now as it does not seem worth the while to explain prolixly in the common manner, what my *Analysis* does briefly'. This quadrature, compos'd in the common manner, he began to communicate at *Paris* in 1675; the following year he was polishing the demonstration thereof to send it to Mr. *Oldenburgh*, by way of return for Mr. *Newton's* method, as he signifies in a letter of May 12. 1676, and accordingly in his letter of August 27. 1676, he sent it compos'd, and polish'd in the common manner; The following winter he returned into *Germany*, by *England* and *Holland*, to enter upon publick business, and had no longer any leisure to fit it for the press, nor thought it afterwards worth his while to explain those things prolixly in the common manner, which his new *Analysis* exhibited in short; he, therefore found out this new method after his return into *Germany*, and consequently, not before 1679.

The same is farther manifest by the following consideration; Dr. *Barrow* publish'd his method of tangents in 1670; from which Mr. *Gregory* deduced his method of tangents without computation, as he wrote Mr. *Collins* September 5. 1670; Mr. *Newton* in a letter, dated December 10. 1672, communicated his *Method of Tangents* to Mr. *Collins*, and added; 'this is one particular, or rather corollary of the general method, that extends without any troublesome calculation, not only to the drawing of tangents to any kind of curves, either geometrical, or mechanical, or any how regarding right lines, or other curves; but likewise to the solving of other more abstruse problems of curvatures, area's, lengths, centres of gravity of curves, &c. nor is it confined (like *Huddenius's* method *de maximis & minimis*) only to such equations, as have no surd quantities; I have added this method to that other, wherein I give an exegesis of equations, by reducing them into infinite series'. M. *Slussus* sent his *Method of Tangents* to Mr. *Oldenburgh* January 17. 1674, and the same was soon after publish'd in the *Philosophical Transactions*; it prov'd to be the same with that of Mr. *Newton's*, it was

founded upon three *Lemma's*, the first of which was; the difference of two powers of the same degree, divided by the difference of the sides, or roots gives the members of the inferior degree of the binominal of the sides, as $\frac{y^3 - x^3}{y - x} = yy + yx$ + x^2 , that is, in M. *Leibnitz's* notation $\frac{dy^3}{dy} = 3yy$. A

copy of Mr. *Newton's* letter of December 10. 1672, was sent to M. *Leibnitz* by Mr. *Oldenburgh*, amongst Mr. *James Gregory's* papers, at the same time with Mr. *Newton's* letter of June 13. 1676. And Mr. *Newton* having mentioned in these two letters, that he had a very general *Analysis*, partly consisting of the method of converging series, and partly of another method, whereby he applied those series to the solution of almost all problems, (excepting perhaps some numeral ones, like those of *Diophantus*) and found out the tangents, area's, lengths, solid contents, centres of gravity and curvities of curves, and curvilinear figures, either geometrical or mechanical, without sticking at surds; and that *Slusius's* method of tangents was but a branch or corollary of this other method; M. *Leibnitz*, having seen these in his return home through *Holland*, was then only meditating the improvement of *Slusius's* method; for, in a letter to Mr. *Oldenburgh*, dated from *Amsterdam*, November 24. 1676, he wrote thus; 'the method of tangents, publish'd by *Slusius*, does not reach so far but that something farther might be done in that kind, that would be of very great use in all sorts of problems, even in my method (without extractions) of reducing equations to series; to wit, a certain short table of tangents might be calculated and continued so far, till its progression appear, so that any one might continue it as far as he pleased, without any calculation'. This was the improvement of *Slusius's* method into a general one, which M. *Leibnitz* was then meditating, and by his words, *Something farther might be done in that kind, that would be of very great use in all sorts of problems*; this seems to be the only improvement, by which he would extend that method to all sorts of problems; but the improvement by the differential *Calculus* was not yet thought of, for that must be refer'd to the following year.

Mr. *Newton* in his next letter, dated October 24. 1676, mentioned the *Analysis*, communicated by Dr. *Barrow* to Mr. *Collins* in 1669, and also another tract written in 1671, about converging series, and about the other method, whereby tangents were drawn

drawn according to that of *Slusius*, and the *Maxima* and *Minima* determined, and the quadrature of curves made more easy, and this without sticking at radicals; and by which also *Series* were invented, which in certain cases broke off, and gave the quadrature of curves in finite equations, when it might be done; and he comprehended the foundation of these operations in this sentence, expressed enigmatically as above, 'having an equation given that involves any number of flowing quantities to find their fluxions, and *vice versa*.' Which puts it past all dispute, that he had invented the method of fluxions before that time; and if other things in that letter be considered, it will appear, that he had then brought it to great perfection, and made it exceeding general; since the propositions in his book of quadratures, and the methods of converging *Series*, and of drawing a curve line thro' any number of given points, were then known to him; for, when the method of fluxions does not proceed in finite equations, he reduces the equations into converging *Series* by the binomial theorem, and by the extraction of fluents out of equations, either involving, or not involving their fluxions; and when finite equations are wanting, he deduces converging *Series* from the conditions of the problem, by assuming the terms of the *Series* gradually, and determining them by those conditions; and when fluents are to be deduced from fluxions, and the law of the fluxions is wanting, he finds that law very nearly, by drawing a parabolic line thro' any number of given points; and by these improvements Mr. *Newton* had in those days made his method of fluxions much more universal than the differential method of M. *Leibnitz* is at present: This letter of Mr. *Newton*'s, dated *October* 24. 1676 was seen by M. *Leibnitz* at *London* in the latter end of the winter, or beginning of the spring following, and he had a copy of it at *Hanover* the beginning of the ensuing spring; and M. *Leibnitz* soon after replied in a letter, dated *June* 21. 1677; 'I agree with Mr. *Newton*, that *Slusius*'s method of tangents is still imperfect; and I have long ago handled the subject more generally, *viz.* by the differences of the ordinates' — hence, for the future, calling dy the difference of the two nearest y , &c. Here at length M. *Leibnitz* first began to propose his differential method, and there is not the least evidence, that he knew it before the receipt of Mr. *Newton*'s last letter; he says indeed, 'that he had a long time ago handled the subject of tangents more generally, *viz.* by the differences of the ordinates,' and so he had affirmed before in other letters, that he had invented several converging *Series*, both direct, and
inverse,

inverse, before he had any method of inventing them; and had forgot an inverse method of *Series* before he perceived its usefulness; but no man is allowed a witness in his own cause, and therefore it lies upon M. *Leibnitz* to prove, that he found out this method before the receipt of Mr. *Newton's* letters; and if he cannot prove this, the question, who was the first inventor of the method, is decided.

The Marquis *De l'Hospital*, in the preface to his *Analyse des infiniment petits*, published in 1696. tells us, 'that a little after the publication of *Des Cartes's* method of tangents, M. *Fermat* also found a method, which *Des Cartes* himself at length allowed to be generally more simple than his own; but it was short of the simplicity Dr. *Barrow* afterwards gave it, by considering more nearly the nature of polygons; which offers naturally to the mind a small triangle, consisting of a small portion of the curve, lying between two ordinates infinitely near each other, and of the difference of these two ordinates, and of that of the two correspondent *Abscissa's*; and this triangle is similar to that, made by the tangent, the ordinate, and the subtangent; so that by one simple analogy, this last method saves all the calculation, which was requisite either in the method of *Des Cartes*, or even in this same method before: Dr. *Barrow* stopp not here, but also invented a sort of calculation proper for this method; but it was necessary, in this as well as in that of *Des Cartes*, to take away fractions, and radicals, to make it useful; therefore, on account of the defect of this *Calculus*, that of M. *Leibnitz* was introduced, who began where Dr. *Barrow* and others left off; this *Calculus* of his led into regions hitherto unknown, and made discoveries there, which surpris'd the ablest mathematicians of *Europe, &c.* Thus far the Marquis; for he had not then seen Mr. *Newton's* *Analysis*, nor his letters of December 10. 1672, June 13. 1676, and October 24. 1676, none of which were published before the year 1699; and so not knowing that Mr. *Newton* had done all this, and signified it to M. *Leibnitz*, the Marquis imagined M. *Leibnitz* began, where Dr. *Barrow* left off, and by teaching how to apply Dr. *Barrow's* method without sticking at fractions, and surds, had wonderfully enlarged the method: And M. *James Bernoulli* in *Acta Erudit.* for January 1691, p. 14. writes to the following purpose; 'whoever understands Dr. *Barrow's* *Calculus* (which he sketched out in his geometrical lectures, and of which all the propositions, therein contained, are specimens) can scarce be ignorant of that other, invented by M. *Leibnitz*, in regard it is founded upon

upon the former, and differs not from it, unless perhaps, in the notation of differentials, and some *Compendia* in the operation.

Now Dr. Barrow, in his method of tangents, draws two ordinates indefinitely near each other, and puts the letter a for the difference of the ordinates, and the letter e for the difference of the *Abscissa's*, and gives these three rules in drawing tangents.

1. In computing, says he, I cast away all the terms in which the power of a or e is found, or in which they are multiplied into themselves; for, these terms will become inconsiderable.

2. After constituting the equation, I cast away all the terms, consisting of symbols, that denote known or determinate quantities, or in which either a or e is not found; for, these terms, being always brought to one side of the equation, will be equal to nothing.

3. I substitute the ordinate for a , and the subtangent for e ; hence at length the quantity of the subtangent will be known. Thus far Dr. Barrow.

And M. Leibnitz in his letter of June 21, 1677 above mentioned, wherein he first began to propose his differential method, has exactly followed this method of tangents, only that he has changed Dr. Barrow's symbols a and e into dx and dy ; for in the example he gives there, he draws two parallel lines, and sets all the terms below the under line, in which dx and dy are, severally or jointly, of more than one dimension; and all the terms in which dx and dy are wanting, above the upper line, and for the reasons given by Dr. Barrow, he makes all these terms vanish; and by the terms in which dx and dy are but of one dimension, and which he sets between the two lines, he determines the proportion of the subtangent to the ordinate; well, therefore, did the Marquis De l'Hospital observe, that where Dr. Barrow left off, there M. Leibnitz began; for, their methods of tangents are exactly the same: But M. Leibnitz observes farther of this method, that the conclusion of this *Calculus* coincides with *Susius's* rule, and that that rule would presently occur to any one, who understood this method; an extraordinary discovery sure! for, Mr. Newton had represented in his letters, that this rule was only a corollary of his general method: And whereas Mr. Newton had said, that his method in drawing of tangents, and determining *Maxima* and *Minima*, &c. proceeded without sticking at surds; M. Leibnitz would also observe, how his method of tangents might be improved so as not to stick either at surds, or fractions, and then would add, I suppose, what Mr. Newton would conceal about the method of drawing of tangents does not differ from this; and what confirms me in this is, that he adds,

that

that from the same foundation quadratures may likewise be rendered more easy; for, always every figure is quadrable, whose ordinate drawn into the difference of the *Abscissa* becomes the difference of any quantity. By which words compared with the preceding calculation, it is manifest, M. *Leibnitz* at this time understood, that Mr. *Newton* had a method of effecting all these things, and that he had been examining, whether Dr. *Barrow's* differential method of tangents might not be extended to the same performances: In November 1684. M. *Leibnitz* published the elements of this differential method in the *Acta Erudit.* which he illustrated with examples of drawing tangents, and determining *Maxima* and *Minima*, and then added, 'and these indeed, are the rudiments of a certain kind of sublimer geometry, that reaches even to the most difficult, and curious problems of mixed mathematics, and which, without the differential Calculus, or some such method, are not readily to be attempted.' The words *some such method* plainly relate to Mr. *Newton's*; and that whole period contains nothing more than what Mr. *Newton* had affirmed of his general method in his letters of 1672 and 1676. And in the *Acta Erudit.* of June 1686. p. 297. M. *Leibnitz* added, 'I choose rather to make use of dx and the like than of single letters, in their stead, because dx is a certain kind of modification of x itself, &c.' He knew very well that in this method he might conveniently enough have used single letters with Dr. *Barrow*, but he chose rather to use the new symbols dx and dy , tho' nothing can be done by them, but what with more conciseness and advantage may be done by single letters.

The next year Mr. *Newton's Principia Philosoph.* came out, a book full of such problems, as M. *Leibnitz* called *the most difficult and curious*, &c. And the Marquis *De l'Hospital* speaks thus of it; 'it is almost entirely composed by this Calculus': And M. *Leibnitz* himself in a letter to Mr. *Newton*, dated from *Hanover March 27*, 1693, and still extant in his own hand writing, and exhibited to the *Royal Society*, acknowledged the same thing in these words; 'you had surprisingly enlarged geometry by your *Series*, but by your *Principia* you have shewn, that you have penetrated into what was beyond the reach of the common *Analysis*; I have likewise attempted, by making use of proper symbols to express the sums and differences, to reduce in some measure to *Analysis*, that geometry I call the 'transcendental, and I have not been unsuccessful.' And again in his answer to M. *Fatio*, printed in the *Acta Erudit.* of May

1700. p. 203. *lin.* 21. he acknowledged the same thing. In the second *Lemma* of the second book of these *Principia* the elements of this *Calculus* are demonstrated synthetically, and at the end of the *Lemma* there is a *Scholium* in these words; 'when I signified in the letters, that passed 10 years ago between me and M. *Leibnitz*, that I had a method of determining *Maxima* and *Minima*, of drawing tangents, &c. that succeeded as well in surd as in rational terms; and concealed the same by transposing the letters including this proposition, *having an equation given, that involves any number of fluents or flowing quantities, to find the fluxions, and vice versa*; M. *Leibnitz* replied that he had likewise hit on such a method, and which he communicated to me, scarcely differing from mine, except in the form of the words and symbols; the foundation of both is contained in this *Lemma*.' In those letters and in another, dated December 10. 1672, a copy of which was sent to M. *Leibnitz* by Mr. *Oldenburgh*, as is mentioned above, Mr. *Newton* had so far explained his method, that it was not difficult for M. *Leibnitz*, by the help of Dr. *Barrow's* method of tangents to collect it from those letters; and it is certain by the arguments above-mentioned, that he did not know it before the writing of those letters.

Dr. *Wallis* had received copies of Mr. *Newton's* two letters June 13. and October 24. 1676, from Mr. *Oldenburgh*, and published several things out of them in his *Algebra*, printed in English 1683, and in Latin 1693; and soon after, he was advised from *Holland* to print the letters entire, because Mr. *Newton's* notions of fluxions passed there with applause by the name of the differential method of M. *Leibnitz*; and thereupon he took notice of this matter in the preface to the first volume of his works, published 1695, and in a letter to M. *Leibnitz*, dated December 1. 1696, and which is extant in the third volume of Dr. *Wallis's* works, he gave the following account of it; 'after the last sheet of the preface had been composed at the press, a certain friend of mine, skilled in such matters, and who happened to be abroad at that time, acquainted me, that such a method was then spoken of in *Holland*, and also that it nearly coincided with Mr. *Newton's* method of fluxions, which made me give an intimation of it.' And in a letter, dated April 10. 1695, and lately communicated to the *Royal Society*, he writes thus; 'I wish you would print the two large letters of June and August (he means June and October) 1676; I had intimation from *Holland*, that your friends there require it; because,

' your motions of fluxions pass there with great applause by the
 ' name of *Leibnitz's calculus Differentialis*. You are not so
 ' kind to your reputation, and that of the nation as you might
 ' be, when you let things of worth lie by you so long, till others
 ' carry away the reputation that is due to you; I have endea-
 ' voured to do you justice in that point, and am now sorry that
 ' I did not print those two letters *Verbatim*. The short intima-
 ' tion of this matter, which Dr. Wallis inserted into the said pre-
 ' face, was to the following purpose; amongst other things in the
 ' second volume there is Mr. Newton's method of fluxions, as
 ' he calls it, of a like nature with M. Leibnitz's *calculus diffe-*
 ' *rentialis*, as he terms it (as any one who compares both me-
 ' thods will easily find, only under different forms of expression)
 ' which I have described *cap. 91, &c.* especially *cap. 93.* from
 ' Mr. Newton's two letters, or one of them, dated *June 13.* and
 ' *October 24. 1676.* written to Mr. Oldenburgh, and to be com-
 ' municated to M. Leibnitz (almost in the same words, or at
 ' least with little variation, from what is contained in the said
 ' letters) where *he explains this method to M. Leibnitz*, which
 ' he had invented about 10 years before, if not more, that is in
 ' 1666 or 1665; which I hint, that none may alledge, I have
 ' said nothing of this *Calculus differentialis*.

Upon this, the editors of the *Acta Erudit.* for June, the
 following year, or rather, as may be gathered from the stile,
 M. Leibnitz himself in giving an account of these two first vo-
 lumes of Dr. Wallis, took notice of this clause of the Dr's pre-
 face, and complained, not of his saying, that Mr. Newton in his
 two letters above-mentioned explained to M. Leibnitz the me-
 thod of fluxions, found by him 10 years before, or better; but
 that when the Dr. mentioned the differential *Calculus*, and said,
 that he did it, ' that none might alledge, that he had said no-
 ' thing about the differential *Calculus*, he did not tell the reader,
 that M. Leibnitz was master of this *Calculus* at that time, when
 those letters passed between him and Mr. Newton, by means of
 Mr. Oldenburgh. And in several letters, which followed here-
 upon between M. Leibnitz, and Dr. Wallis concerning this mat-
 ter, M. Leibnitz did not deny, but that Mr. Newton had the
 method upwards of 10 years before the writing of those letters,
 as Dr. Wallis had affirmed; and did not pretend, that he him-
 self had his own method so early; brought no proof, that he
 had it before 1677, no proof even for that, besides Mr. Newton's
 concession; did not affirm, that he had it earlier; commended
 Mr. Newton for his candour in this matter; allowed, that the
 methods

methods agreed in the main; and said, that he, therefore, used to call them by the common name of the *infinitesimal Analysis*; represented, that as the methods of *Vieta* and *Carpes* were called by the common name of *Analysis Speciosa*, and yet differed in some things, so, perhaps, Mr. *Newton's* method and his own, might differ in some things; and he challenged to himself only those things wherein, as he conceived, they might differ, as the notation, the differential and exponential equations; but in his letter of *June 21. 1677*, he reckoned the differential equations common to Mr. *Newton*, and himself.

This was the state of the controversy between Dr. *Wallis* and Mr. *Leibnitz* at that time; but four years after, when M. *Fatio* suggested, that Mr. *Leibnitz*, the second inventor of this *Calculus*, might borrow some things from Mr. *Newton*, the oldest inventor by many years; Mr. *Leibnitz* in his answer, published in the *Acta Erudit.* of *May 1700*, allowed, that Mr. *Newton* had found the method apart, and did not deny, that Mr. *Newton* was the oldest inventor by many years, nor assumed any thing more to himself than that he also had found the method apart, or without the assistance of Mr. *Newton*, and pretended, that when he first published it, he knew not that Mr. *Newton* had found any thing more of it than the method of tangents; and in making this defence he added, 'which method, no geometrician that I know of, had before Mr. *Newton* and me, as none before him gave a public specimen thereof, and none before both the M. *Berhoullii's* and me communicated it'. Hitherto, therefore, Mr. *Leibnitz* did not pretend to be the first inventor; he did not begin to put in such a claim, till after the death of Dr. *Wallis*, the last of the old men, who were acquainted with what had passed between the *English* and Mr. *Leibnitz* for 40 years before; the Dr. died in *October 1703*, and Mr. *Leibnitz* did not begin to put in this new claim before *January 1705*.

Mr. *Newton* published his *treatise of quadratures* in 1704, which was written long before, several things being cited out of it in his letters of *October 24.* and *November 8. 1676*; it relates to the method of fluxions, and that it might not be taken for a new piece, Mr. *Newton* repeated what Dr. *Wallis* had published nine years before, without being then contradicted, namely, that this method was invented by degrees in 1665 and 1666: Hereupon, the editors of the *Acta Erudit.* in *Jan. 1705*, or rather Mr. *Leibnitz* himself, in giving an account of this book, represented, that Mr. *Leibnitz* was the first inventor of the method,

and that Mr. Newton had substituted fluxions for differences; and this assertion gave rise to the present controversy; for, Mr. Keill in a letter, publish'd in the *Philosophical Transactions* for September and October 1708, retorted the accusation, asserting, that Mr. Newton was, beyond all dispute, the first inventor of the arithmetic of fluxions, as would easily appear to any one that reads his letters, publish'd by Dr. Wallis; tho' the same method, by only changing the name and manner of notation, was afterwards publish'd by M. Leibnitz in the *Acta Erudit.*

Before Mr. Newton saw what had been publish'd in the *Acta Erudit.* he express'd himself offended at the printing of this paragraph of Mr. Keill's letter; lest it should cause a dispute; and M. Leibnitz, putting a worse construction on it than Mr. Keill intended, complain'd of it as a calumny in a letter to Dr. Sloane, dated March 4. 1711, N. S. and mov'd, that the *Royal Society* would cause Mr. Keill to make a publick recantation: But Mr. Keill insisted he would prove and defend what he had written; and Mr. Newton, upon being shewn the accusation in the *Acta Erudit.* gave him leave so to do; but M. Leibnitz, in a second letter to Dr. Sloane, dated December 29. 1711, instead of making good his accusation, as he was bound to do, that it might not be deemed a calumny, insisted only upon his own candour, as if it would be injustice to question it; and refus'd to tell how he came by the method; and said that the *Acta Erudit.* had given every man his due, and that he had conceal'd the invention upwards of nine years (he should have said seven years) that no body might pretend (he means that Mr. Newton might not pretend) to have been before him in it; and call'd Mr. Keill a novice, unacquainted with things past, and one that acted without authority from Mr. Newton, and a clamorous man who deserv'd to be silenced; and desir'd, that Mr. Newton himself would give his opinion in the matter: He knew, that Mr. Keill affirm'd nothing more than what Dr. Wallis had publish'd 13 years before, without being then contradicted: He knew, that Mr. Newton had given his opinion in this matter in the introduction to his book of *Quadratures*, publish'd before this controversy began; but Dr. Wallis was now dead; the surviving mathematicians in *England* are reckoned novices; M. Leibnitz would assume to himself to question any man's candour without injustice, and Mr. Newton, unless he would either dissemble or deny the matter, must be involved in wrangling and disputes.

The *Royal Society*, therefore, having as much authority over *M. Leibnitz*, as over *Mr. Keill*, as being two of their members, and being now twice pressed by *M. Leibnitz* to interpose, and thinking it unjust either to condemn, or censure *Mr. Keill* without enquiring into the matter; and knowing, that neither *Mr. Newton*, nor *M. Leibnitz* (the only persons alive who knew and remember'd any thing of what had pass'd in these matters 40 years before) could be witnesses for, or against *Mr. Keill*, appointed a numerous *Committee* to search old letters and papers, and report their opinion upon what they should find; and as soon as this report was made, they order'd the letters and papers, with the report of their *Committee*, to be publish'd; and by these letters, and papers it appeared, that *Mr. Newton* had the method in or before the year 1669, and that *M. Leibnitz* had it not before the year 1677.

M. Leibnitz, to make himself the first inventor of the differential method, represented, that *Mr. Newton* at first us'd the letter o in the vulgar manner for the given increment of x , which destroys the advantages of the differential method; but after the writing of his *Principia*, changed o into $\dot{x}$, substituting $\dot{x}$ for dx : It lies upon *M. Leibnitz* to prove, that *Mr. Newton* ever changed o into $\dot{x}$, or us'd $\dot{x}$ for dx , or left off the use of the letter o : *Mr. Newton* us'd the letter o in his *Analysis*, written in or before the year 1669, and in his book of *Quadratures*, and in his *Principia Philos.* and still uses it in the very same sense as at first: In his book of *Quadratures* he us'd it in conjunction with the symbol $\dot{x}$, and, therefore, did not use that symbol in its room; these symbols o and $\dot{x}$ are put for things of a different kind; the one is a moment, and the other is a fluxion, or velocity, as has been explained above: When the letter x is put for a quantity which flows uniformly, the symbol $\dot{x}$ is an unit, and the letter o a moment, and $\dot{x} o$ and $d x$ signifying the same moment; prick'd letters never signify moments, unless when they are multiplied by the moment o , either express'd, to make them infinitely little, and then the rectangles are put for moments.

Mr. Newton does not place his method in forms of symbols, nor confine himself to any particular sort of symbols for fluents and fluxions; where he puts the area's of curves for fluents, he frequently puts the ordinates for fluxions, and denotes the fluxions by the symbols of the ordinates, as in his *Analysis*; where he puts lines for fluents, he puts any symbols for the velocities of the points, which describe the lines, that is, for the first fluxions; and any other symbols for the increase of those velocities, that is, for

for the second fluxions, as is frequently done in his *Principia Philof.* and where he puts the letters, x, y, z for fluents, he denotes their fluxions, either by other letters, as p, q, r ; or by the same letters in other forms, as $\dot{x}, \dot{y}, \dot{z}$, or $\dot{x}, \dot{y}, \dot{z}$; or by any lines, as DE, FG, HI , consider'd as their exponents; and this is evident from his book of *Quadratures*, where he represents fluxions by prick'd letters in the first proposition, by ordinates of curves in the last proposition, and by other symbols in explaining the method, and illustrating it with examples in the introduction: M. Leibnitz has no symbols of fluxions in his method; and, therefore Mr. Newton's symbols of fluxions are the oldest in the kind: M. Leibnitz began to use the symbols of moments, or differences dx, dy, dz , in the year 1677: Mr. Newton represented moments by the rectangles, under the fluxions and the moment o , when he wrote his *Analysis* in, or before 1669: M. Leibnitz us'd the symbols $\int x, \int y, \int z$, for the sums of ordinates ever since the year 1686; Mr. Newton represented the same thing in his *Analysis*, by inscribing the ordinate in a square or rectangle, in this manner $\frac{aa}{64x}$: All Mr. Newton's symbols

are the oldest in their several kinds by many years. And whereas it has been represented, that the use of the letter o is vulgar, and destroys the advantages of the differential method; on the contrary, the method of fluxions, as us'd by Mr. Newton, has all the advantages of the differential, and besides some others; it is more elegant, because in this *Calculus* there is but one infinitely small quantity represented by a symbol, which is o : We have no ideas of infinitely small quantities, and therefore, Mr. Newton introduced fluxions into his method, that it might proceed by finite quantities as much as possible; it is more natural, and geometrical, because founded upon the prime ratios of nascent quantities, which have a being in geometry, whilst indivisibles on the contrary, on which the differential method is founded, have no being, either in geometry, or nature; there are, it is true, prime ratios of nascent quantities; but no prime nascent quantities: Nature generates quantities by continual flux, or increase; and the ancient geometers admitted such a generation of area's and solids, when they drew one line into another by local motion to generate an area, and the area into a line by local motion to generate a solid; but the summing up of indivisibles to compose an area, or solid, was never yet admitted into geometry: Mr. Newton's method is also of greater use, and certainty,

tainty, being adapted either to the ready finding out of a proposition by such approximations, as will create no error in the conclusion, or to the demonstrating it exactly; M. *Leibnitz's* method is only for finding it out; when the work does not succeed in finite equations, Mr. *Newton* has recourse to converging series, and thereby his method becomes incomparably more universal than that of M. *Leibnitz*, which is confined to finite equations; for, he has no share in the method of infinite series. Some years after the method of series was invented, M. *Leibnitz* found out a proposition for the transmutation of curvilinear figures into others of equal area's, in order to square them by converging series; but the methods of squaring those other figures by such series were not his: By the help of the new *Analysis* Mr. *Newton* found out most of the propositions in his *Principia Philos.* but because the ancients, for making things certain, admitted nothing into geometry before it was demonstrated synthetically, Mr. *Newton* therefore demonstrated the propositions synthetically, that the system of the heavens might be founded upon good geometry; and this makes it now difficult for the unskilful to see the *Analysis* by which those propositions were found out.

It has been represented, that Mr. *Newton*, in the *Scholium* at the end of his book of *Quadratures*, has put the third, fourth, and fifth terms of a converging series, respectively equal to the second, third, and fourth differences of the first term, and therefore did not then understand the method of second, third, and fourth differences; but, in the first proposition of that book, he shew'd how to find the first, second, third, and following fluxions in infinitum; and, therefore, when he wrote that book, which was before the year 1676, he understood the method of finding all the fluxions, and consequently all the differences; and if he did not understand it, when he added that *Scholium* to the end of the book in 1704, it must have been, because he had then forgot it in that long tract of time; and so the question is only, whether he had forgot the method of second and third differences before the year 1704.

In the tenth proposition of the second book of his *Principia*, in describing some of the uses of the terms of a converging series for solving of problems, he tells us, that if the first term of the series represent the ordinate BC in Fig. 6. of any curve line ACG , and $CBDI$ be a parallelogram infinitely narrow, whose side DI cuts the curve in G , and its tangent CF in F , the second term of the series will represent the line IF , and the third term the line FG ; now the line FG is but half the second difference of

of the ordinate; and therefore Mr. *Newton*, when he wrote his *Principia*, put the third term of the series equal to half the second difference of the first term; and, consequently, had not then forgotten the method of second differences: In writing that book, he had frequent occasion to consider the increase, or decrease of the velocities with which quantities are generated, and argues right about it; that increase or decrease is the second fluxion of the quantity, and therefore, he had not then forgotten the method of second fluxions: In 1692 Mr. *Newton*, at the request of Dr. *Wallis*, sent him a copy of the first proposition of the book of quadratures, with examples thereof in first, second and third fluxions, as may be seen in the second volume of the Dr's works p. 391, 392, 393 and 396. and therefore, he had not then forgot the method of second fluxions: Nor is it likely, that in 1704, when he added the aforesaid *Scholium* to the end of the book of quadratures, he had forgot not only the first proposition of that book, but also the last proposition, to which that *Scholium* was subjoined; if the word *ut*, which in that *Scholium* may have been accidentally omitted between the words *erit* and *ejus*, be restored, that *Scholium* will agree with the two propositions, and with the rest of his writings, and the objection will vanish.

Thus much concerning the nature and history of these methods; next follow some observations thereon: In the *Commercium epistolicum* mention is made of three tracts written by M. *Leibnitz*, after a copy of Mr. *Newton*'s *Principia* had been sent him to *Hanover*, and after he had seen an account of that book, published in the *Acta Erudit.* for *January* and *February* 1689; and in those tracts the principal propositions of that book are composed in a new manner, and claimed by M. *Leibnitz*, as if he had found them himself before the publishing of that book; but M. *Leibnitz* cannot be a witness in his own cause; it lies upon him either to prove, that he found them before the publication of Mr. *Newton*'s book, or to quit his claim: In the last of those three tracts the 20th proposition (which is the chief of Mr. *Newton*'s propositions) is made a corollary of the 19th proposition, and the 19th proposition has an erroneous demonstration to it: It lies upon him, either to satisfy the world, that the demonstration is not erroneous, or to acknowledge, that he did not find that and the 20th proposition thereby, but tried to adapt a demonstration to Mr. *Newton*'s proposition to make it his own; for, he represents in his 20th proposition, that he knew not how Mr. *Newton* came by it, and consequently, that he found it him-
self

self without the assistance of Mr. *Newton*: By the errors in the 15th and 19th propositions of the third tract, Dr. *Keill* hath shewn, that when M. *Leibnitz* wrote these three tracts he did not well understand the ways of working in second differences; and this is farther manifest by the 10th, 11th, and 12th propositions of this third tract; for, he lays down these as the foundation of his infinitesimal *Analysis* in arguing about centrifugal forces, and proposes the first of them with relation to the center of curvity of the orbit, but uses this proposition in the two next, with relation to the center of circulation; and since he confounded these two centers with each other in the fundamental propositions, upon which he grounds this *Calculus*, he must necessarily have erred in the superstructure; and for want of skill in second and third differences, was not able to extricate himself from these errors; and this is farther confirmed by the sixth article of the second tract; for, that article is erroneous, and the error arises from his not knowing how to argue well about second and third differences; when therefore, he wrote those tracts he was but a learner, and this he ought in candour to acknowledge.

It seems therefore, that as he learned the differential method by means of Mr. *Newton's* aforesaid three letters, compared with Dr. *Barrow's* method of tangents; so 10 years after, when Mr. *Newton's Principia* came abroad, he improved his knowledge in these matters, by trying to extend this method to the principal propositions in that book, and by that means composed the said three tracts; for, the propositions contained therein (errors and trifles excepted) are Mr. *Newton's* (or easy corollaries from them) being published by him in other forms of words before, tho' to the same purpose; and yet M. *Leibnitz* published them as invented by himself long before they were published by Mr. *Newton*; for, in the latter end of the first tract he represents that he invented them all before Mr. *Newton's Principia* came abroad, and some of them before he left *Paris*, that is, before *October* 1676: And the second tract he concludes with words to the following purpose; 'from what has been advanced a great many things might be deduced, accommodated to practice, but it shall have now sufficed, that I have laid down geometrical principles, in which consisted the greatest difficulty; and perhaps, I may seem to an attentive considerer to have opened some new ways, that were pretty intricate before; for, every thing answers to my *Analysis* of infinites, that is, to the *Calculus* of sums and differences (some of whose elements I have given in the *Acta Erudit.*) which I have here expressed in

‘as plain a manner as the thing would bear.’ He pretends here, that the geometrical principles, in which consisted the greatest difficulty, were first laid down by himself in this very tract, and that he himself had in this very tract opened some new ways, that were pretty intricate before; and yet Mr. Newton’s *Principia* came out almost two years before, and was of service to him in polishing this very tract, and was written in as plain a manner, as the thing would bear, and contained all those principles, and all those new ways; and M. Leibnitz, when he published that tract, knew all this, and therefore, ought then to have acknowledged, that Mr. Newton was the first who laid down the geometrical principles, in which consisted the greatest difficulty, and opened the new ways, &c. And it is true, that in his answer to M. Fatio he acknowledged all this, saying, ‘which method, no geometrician that I know of, had before Mr. Newton and me; as none, before this celebrated geometrician, gave a public specimen that he had it.’ And what he then acknowledged, he ought in candour and honour to acknowledge still on all occasions.

M. Leibnitz in his letter of May 28th, 1697. wrote thus to Dr. Wallis; ‘that Mr. Newton’s method of fluxions had an affinity with my differential method, I not only perceived, after his book of *Principia*, and your book were published; but I likewise acknowledged so much in the *Acta Erudit.* and on other occasions; for, I judged, that this became my own candor, as well as his merit; therefore, I usually call them both by the common name of *Analystis infinitesimalis*, which is more extensive than the *Tetragonistica*; for, as *Vieta*’s and *Des Cartes*’s methods are called *Analysis speciosa*, tho’ there is some difference between them; so perhaps, Mr. Newton’s method and mine may differ in some things.’ Here also M. Leibnitz allows, that when Mr. Newton’s *Principia* came out, he was apprised of their affinity, and therefore, called them both by the common name of the infinitesimal method, and thought himself bound in candour to acknowledge this affinity; and there is still the same obligation upon him in point of candour; and besides this acknowledgement he here gives the preference to Mr. Newton’s method in point of antiquity; for, he represents, that as the vulgar *Analysis* in species was invented by *Vieta*, and augmented by *Cartes*, which made some differences between their methods; so Mr. Newton’s method and his own might differ in some things; and then he goes on to enumerate the differences by which he had improved Mr. Newton’s method, as was mentioned above;

above; and this subordination of his method to Mr. *Newton's*, which he then acknowledged to Dr. *Wallis*, he ought still to acknowledge.

In enumerating the differences, or improvements which he had made to Mr. *Newton's* method, he mentions in the second place differential equations; but the letters, which passed between them in 1676, shew, that Mr. *Newton* had such equations at that time, and that M. *Leibnitz* had them not; he mentions in the third place exponential equations, but these equations are owing to his correspondence with the *English*: Dr. *Wallis* in the interpolation of *Series* considered fractional and negative indices of powers; Mr. *Newton* introduced into his analytical computations fractional, surd, negative and indefinite indices of powers; and in his letter of *October 24. 1676*, represented to M. *Leibnitz*, that his method extended to the resolution of affected equations, involving powers whose indices were fractional or surd: M. *Leibnitz* in his answer, dated *June 21. 1677*, desired Mr. *Newton* to give him his thoughts on the resolution of equations involving powers, whose indices were indetermin'd, such as these, $x^y + y^x = xy$. $x^x + y^y = x + y$; and these equations he now calls exponential, and represents to the world, that he was the first inventor thereof, and magnifies the invention as a great discovery; but he has not yet made a public acknowledgement of the light which Mr. *Newton* gave him into it, nor produced any one instance of the use that he has been able to make of it, where the indices of powers are fluents; and since he has not yet rejected it with his usual impatience for want of such an instance, we have reason to expect, that he will at length explain its usefulness to the world.

Mr. *Newton* in his letter of *October 24. 1676*, wrote, that he had two methods of resolving the inverse problems of tangents, and such like difficult ones; one of which consisted in assuming a *Series* for any unknown quantity, from which all the rest might be conveniently deduced, and in collating the homologous terms of the resulting equation, for determining the terms of the assumed *Series*: M. *Leibnitz* several years after published this method as his own, claiming to himself the first invention thereof; it remains, that he either publicly renounce his claim, or prove, that he invented it before Mr. *Newton* wrote his letter: It also lies upon him to make a public acknowledgement of his receipt of Mr. *Oldenburgh's* letter of *April 15. 1675*. wherein several converging *Series* for squaring of curves, and particularly, that of Mr. *James Gregory* for finding the arch by the given tan-

gent and thereby squaring the circle, were communicated to him: He acknowledged it privately in his letter to Mr. Oldenburgh, dated *May 20. 1675*, still extant in his own hand-writing, and entered by Mr. Oldenburgh in the letter-book of the *Royal Society*; but he has not yet acknowledged it publicly, as he ought to have done, when he published that *Series* as his own: It also lies upon him to make a public acknowledgement of his having received the extracts of Mr. James Gregory's letters, which at his own request were sent to him at *Paris* in *June 1676* by Mr. Oldenburgh to peruse; amongst which was Mr. James Gregory's letter of *February 15. 1671*, concerning that *Series*, and Mr. Newton's letter of *December 10. 1672*, concerning the method of fluxions.

And whereas in his letter of *December 28. 1675*, he wrote to Mr. Oldenburgh, that he had communicated that *Series* upwards of two years before to his friends at *Paris*, and had written to him sometimes about it; and in his letter of *May 12. 1676* to Mr. Oldenburgh, he said that he had written to him about that *Series* some years before; and in his letter to Mr. Oldenburgh, dated *August 27. 1676*, that he had communicated that *Series* to his friends upwards of three years before, that is, before his first coming from *London* to *Paris*; he is desired to tell how it came to pass, that when he received Mr. Oldenburgh's letter of *April 15. 1675*, he did not know that *Series* to be his own: In his letters of *July 15. and October 26. 1674*, he tells us but of one *Series* for the circumference of a circle, and saith, that the method, which gave him this *Series* gave him also a *Series* for any arch whose sine was given, tho' the ratio of the arch to the whole circumference be unknown; this method therefore, by the given sine of 30 degrees, gave him a *Series* for the whole circumference; if he also had a *Series* for the whole circumference, deduced from the tangent of 45 degrees, he is desired to tell the world what method he had in those days, which could give him both those *Series*; for the method by the transmutation of figures will not do it; he is also desired to tell, why in his said letters he did not mention more quadratures of the circle than one.

And if in the year 1674 he had the demonstration of a *Series* for finding any arch whose sine is given, he is desired to tell the world what it was; and why in his letter of *May 12. 1676* he desired Mr. Oldenburgh to procure from Mr. Collins the demonstration of Mr. Newton's series for doing the same thing, and wherein his own series differ'd from Mr. Newton's; for, from all these considerations there is no small suspicion, that Mr. Newton's series

for finding the arch, whose sine is given, was communicated to him in *England*, and that in 1673 he began to communicate it as his own to some of his friends at *Paris*, and the next year wrote of it as his own, in his letters to Mr. *Oldenburgh*, in order to procure the demonstration, or method of finding such series; but the year following, when Mr. *Oldenburgh* sent him this series, and Mr. *Gregory's* series, and six other series, he drop'd his pretension to this series for the want of a demonstration, and took time to consider the series sent him, and to compare them with his own, as if his series were different from those sent him; and when he had found a demonstration of Mr. *Gregory's* series by a transmutation of figures, he began to communicate it as his own to his friends at *Paris*, as he represents in the *Acta Erudit.* for *April*, 1691. p. 178. saying, 'now in 1675 I had by me a little piece I had composed upon the arithmetical quadrature, which from that time was perused by my friends, &c.' But the letter, by which he had received this series from Mr. *Oldenburgh*, he concealed from his friends, and pretended to Mr. *Oldenburgh*, that he had this series a year or two before the receipt of that letter; and the next year upon receiving two of Mr. *Newton's* series again by one *George Mohr*, he wrote to Mr. *Oldenburgh* in such a manner as if he had never seen them before, and upon pretence of their novelty, desired Mr. *Oldenburgh* to procure from Mr. *Collins* Mr. *Newton's* method of finding them; if M. *Leibnitz* thinks fit to obviate this suspicion, he is in the first place to prove that he had Mr. *Gregory's* method before he received it from Mr. *Oldenburgh*.

It also lies upon him to tell the world, what was the method by which the several series of regression for the circle and hyperbola, sent to him by Mr. *Newton* *June* 13. 1676, and claimed as his own by his letter of *August* 27. following, were found by him before he receiv'd them from Mr. *Newton*: And whereas Mr. *Newton* sent him, at his own request, a method of regression, which, upon the first reading he did not know to be his own, nor understood it; but as soon as he understood it, he claimed as his own, by pretending, that he had found it long before, and had forgot it, as he perceiv'd by his old papers; it lies upon him, in point of candour and justice, either to prove that he was the first inventor of this method, or to renounce his claim to it for preventing further disputes.

M. *Leibnitz* in his letter to Mr. *Oldenburgh*, dated *February* 3. 1678, claimed a right to a certain property of a series of numbers natural, triangular, pyramidal, triangulo-triangular, &c. and

and to make it his own, represented, that he wonder'd, how *M. Pascal*, in his book, entitled *Triangulum Arithmeticum*, should omit it: That book was publish'd in 1663, and contains this property of the series; and *M. Leibnitz* has not yet done him the justice to acknowledge, that he did not omit it: It lies upon him, therefore, in candour and justice, to renounce his claim to this property, and acknowledge *M. Pascal* the first inventor: He is also to renounce all right to *M. Mouton's* differential method as second inventor; for, second inventors have no right; the sole right is in the first inventor, till another finds out the same thing a-part; in which case to take away the right of the first inventor, and divide it between him and that other, would be an act of injustice.

In his letter to *Dr. Sloane*, dated *December 29. 1711*, he has told us, that his friends know, how he came by the differential method; it lies upon him, in point of candour, openly and plainly, and without further hesitation, to satisfy the world, how he came by it: In the same letter he has told us, that he had this method upwards of nine years before he publish'd it, that is, in the year 1675, or before; and yet it is certain, that he had it not, when he wrote his letter to *Mr. Oldenburgh*, dated *August 27. 1676*, wherein he affirmed, that problems of the inverse method of tangents and many others, could not be reduced to infinite series, nor to equations or quadratures; it, therefore, lies upon him, in point of candour, to tell us, what he means by pretending to have found the method before he had found it.

We have shewn above, that *M. Leibnitz*, in the latter end of 1676, in his return home from *France thro' England and Holland*, was meditating how to improve *Slusius's* method for tangents, and extend it to all sorts of problems, and for this end propos'd the making a general table of tangents; and, therefore, had not hitherto found out the true improvement; but about half a year after, having newly fallen upon the true improvement, he writ back; 'I agree with the celebrated *Mr. Newton*, that the famous *M. Slusius's* method for tangents is not perfect; and (now 'I have long ago handl'd the subject of tangents in a more general manner, viz. by the differences of the ordinates'; which is as much as to say, that he had this improvement long before those days; and this can be with no other design, than to rival and supplant *Mr. Newton*, and to make us believe, that he had the differential method before *Mr. Newton* explained it to him by his letters of *June 13. and October 24. 1676*, and before
Mr.

Mr. Oldenburgh sent him a copy of Mr. Newton's letter of December 10. 1672, concerning it.

The editors of the *Acta Erudit.* for June 1696, or rather M. Leibnitz himself, in giving an account of the two first volumes of the mathematical works of Dr. Wallis, wrote thus; 'moreover, Mr. Newton himself, no less remarkable for his candour, than great merits in mathematicks, acknowledged both in public and private, when (by means of Mr. Oldenburgh, then Secretary of the Royal Society of London) there was an epistolary correspondence between them, that is, upwards of 20 years before, that M. Leibnitz had his differential Calculus, and infinite series, and also general methods for them; and this Dr. Wallis in the preface to his works, making mention of this correspondence, omitted, because, perhaps, he was not thoroughly acquainted with the matter, besides, M. Leibnitz's method of differences, of which Dr. Wallis makes mention in the following words, viz. that none might alledge, he had said nothing of the differential Calculus, disclosed speculations, that did not equally arise from other principles'.

By this it appears, that M. Leibnitz had seen Dr. Wallis's preface, wherein Mr. Newton is said to have explained to him in 1676, the method of fluxions, which he had discover'd upwards of 10 years before; but Mr. Newton never allow'd, (that M. Leibnitz had the differential method before 1677: and M. Leibnitz himself, in the *Acta Erudit.* for April 1691. p. 178. acknowledged, that he found it after he return'd home from Paris, to enter upon public business, that is, after the year 1676; and as for his pretended general method of infinite series, it is so far from being general, that it is of little or no use; no other use having been made of it, as far as we know, than to colour over the pretence of M. Leibnitz to the series of Mr. Gregory for squaring the circle.

M. Leibnitz in his answer to M. Fatio, printed in the *Acta Erudit.* for 1700. p. 203. writes thus; 'Mr. Newton himself alone, best knows, and sufficiently declared to the publick, when his *Principia* came out in 1687, that some new geometrical discoveries, which were common to us both, were not owing to any light we receiv'd from each other, but to the meditations of each apart, and that they were explained by me 10 years before, i. e. in 1677. In the book of *Principia* here refer'd to Mr. Newton did not acknowledge, M. Leibnitz found this method without receiving light into it from his letters above-mentioned; and Dr. Wallis had lately told him the contrary with-

without being then confuted or contradicted; and if *M. Leibnitz* had found out the method never so much, without the assistance of *Mr. Newton*, yet second inventors have no right: *M. Leibnitz* in his aforesaid answer to *M. Fatio* writes further; 'I can affirm, when in 1684 I publish'd the elements of my *Calculus*, that I did not know any thing more of *Mr. Newton's* inventions in this kind, than what he formerly signified to me by his letters; viz. that he could find tangents, without taking away surds, which *Huygens* afterwards likewise signified to me he could do, tho' I still know no more of that *Calculus*; but at length upon seeing *Mr. Newton's* book of *Principia*, I was fully satisfied, he had made much greater discoveries'. Here he again acknowledged, that the book of *Principia* gave him great light into *Mr. Newton's* method of fluxions, and yet he now denies, that this book contains any thing of that method in it; here he pretended, that before that book came abroad, he knew nothing of *Mr. Newton's* inventions of this kind, than that he had a certain method of tangents, and that by that book he received the first light into *Mr. Newton's* method of fluxions; but in his letter of June 21, 1677, he acknowledged that *Mr. Newton's* method also extended to quadratures of curvilinear figures, and was like his own; his words are to the following purpose: 'I suppose what *Mr. Newton* would conceal, about the method of drawing of tangents, differs not from this, and what confirms me in this opinion is, that he adds, that upon the same foundation quadratures may likewise be rendered more easy; for such figures are always quadrable, whose ordinate drawn into the difference of the absciss becomes the difference of any quantity.

Mr. Newton had in his three letters above-mentioned (copies of which *M. Leibnitz* had received from *Mr. Oldenburgh*) represented his method so general, as by the help of equations both finite and infinite, to determine *Maxima* and *Minima*, tangents, area's, solid contents, centers of gravity, lengths, and curvities of curve lines and curvilinear figures, and this without taking away radicals, and to extend to the like problems in curves, usually called mechanical, and to inverse problems of tangents and others more difficult, and to almost all problems, excepting perhaps, some numeral ones like those of *Diophantus*: And *M. Leibnitz* in his letter of August 27. 1676 represented he could not believe, that *Mr. Newton's* method was so general: *Mr. Newton* in the first of his three letters set down his method of tangents, deduced from this general method, and illustrated it with an example, and said, that this method of tangents was but a branch

a branch or corollary of his general method, and that he took *Slusius's* method of tangents, which then was not published, to be of the same kind; and thereupon, *M. Leibnitz* in his return from *Paris thro' England and Holland* into *Germany*, was considering how to improve *Slusius's* method of tangents, and extend it to all sorts of problems, as was shewn above out of his letters; and *Mr. Newton* in his third letter illustrated his method with theorems for quadratures, and examples thereof; and when he had made so large an explanation of his method, that *M. Leibnitz* had got light into it, and had in his letter of *June 21. 1677*, explained how the method which he was fallen into answered to the description which *Mr. Newton* had given of his method, in drawing of tangents according to *Slusius's* method, proceeding without taking away fractions and surds, and facilitating quadratures; for *M. Leibnitz* to tell the *Germans*, that in *1684* when he first published his differential method, he knew nothing more of *Mr. Newton's* invention than that he had a certain method of tangents, is very extraordinary, and wants an explanation.

At that time *M. Leibnitz* explained nothing more concerning his own method than how to draw tangents, and determine *Maxima* and *Minima*, without taking away fractions, or surds; and he certainly knew, that *Mr. Newton's* method could do all this, and, therefore, ought in candour to have acknowledged it: After he had thus far explain'd his own method, he added, that what he had there laid down, were the principles of a much sublimer geometry, reaching to the most difficult and useful problems, which were scarcely to be resolved without the differential or the like *Calculus*; what he meant by the words *or the like* was impossible for his countrymen the *Germans* to understand without an interpreter; he ought therefore to have done *Mr. Newton* justice in plain intelligible language, and told the *Germans* whole was the *Methodus similis*, and of what extent and antiquity it was, according to the notices he had received from *England*; and to have acknowledged, that his own method was not so ancient; this would have prevented disputes, and nothing less than this could fully deserve the name of candour and justice; but afterwards in his answer to *M. Fatio* to tell the *Germans*, that in *1684*, when he first published the elements of his *Calculus*, he knew nothing of a *Methodus similis*, nothing of any other method than that for drawing of tangents, was very odd and wants an explanation.

It also lies upon *M. Leibnitz* to satisfy the world, why in his answer to *Dr. Wallis* and *M. Fatio*, who had published, that

Mr. *Newton* was the oldest inventor of that method by many years, he did not put in his claim of being the oldest inventor thereof, but staid till the old mathematicians were dead, and then complained of the new mathematicians as novices, attacked Mr. *Newton* himself, and declined to contend with any body else, notwithstanding that Mr. *Newton* in his letter of *October 24. 1676.* had told him, that for the sake of quiet, he had five years before that time laid aside his design of publishing what he had then written on this subject; and ever since he industriously avoided all disputes about philosophical and mathematical subjects, and all correspondence by letters about these matters, as tending to disputes; and for the same reason forbore to complain of M. *Leibnitz*, till it was shewn him, that he stood accused of plagiarism in the *Acta Erudit.* and what Mr. *Keill* had published was only to clear Mr. *Newton* from the guilt of that crime.

It has been said, that the *Royal Society* gave judgment against M. *Leibnitz* without hearing both parties; but this is a mistake; they passed no judgment, they only appointed a *Committee* to search out and examine such old letters and papers as were still extant about those matters, and to report their opinion, how the matter stood according to those letters and papers: The *Committee* was numerous, and skilful, and composed of Gentlemen of several nations, and the *Society* were satisfied in their fidelity in examining the hands and other circumstances, and in printing what they found in the old letters and papers so examined, without adding, omitting, or altering any thing in favour of either party; and the letters and papers are still preserved, that they may be consulted and compared with the *Commercium epistolicum*, whenever desired.

The philosophy Mr. *Newton* has pursued in his *Principia* and optics is experimental; and it is not the business of experimental philosophy to teach the causes of things any further than they can be proved by experiments; we are not to fill this philosophy with opinions which cannot be proved by *Phænomena*; in this philosophy hypotheses have no place, unless as conjectures or questions proposed to be examined by experiments; for this reason Mr. *Newton* in his optics distinguished those things which were made certain by experiments from those things which remained uncertain, and which he, therefore, proposed in the end of his optics in the form of queries; for this reason, in the preface to his *Principia*, when he had mentioned the motions of the planets, comets, moon and sea, as deduced in this book from gravity, he added, ‘I wish the other *Phænomena* of nature could
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by the same way of reasoning be deduced from mechanical principles; for, several things induce me to believe, that all these things may depend upon certain forces, by which the particles of bodies are, by causes still unknown to us, either mutually impelled towards each other, and cohere together according to certain regular configurations, or mutually recede from each other; and for want of knowing these forces philosophers have hitherto attempted to no purpose to explain nature.

And in the end of this book in the second edition, he said, that for want of a sufficient number of experiments, he forbore to describe the laws of the actions of the spirit, or agent by which this attraction is performed; and for the same reason he is silent about the cause of gravity, there occurring neither experiments nor *Phænomena* capable of certainly disclosing the cause thereof; and this he had abundantly declared in his *Principia* near the beginning thereof, to the following purpose; 'I do not now enquire into the physical causes and seats of forces;' and a little after; 'I indifferently and promiscuously use for each other the words attraction, impulse or any kind of propension towards the center, by considering these forces not physically but mathematically; whence I would caution the reader not to think, that by these words I define the species or manner of the action, or the physical cause or reason; or that I truly and physically ascribe forces to centers, which are only mathematical points, if I should happen to say, that either the centers attract, or that there are central forces.' And at the end of his optics; 'here I do not enquire by what efficient cause these qualities, viz. gravity, the magnetic and electrical forces are produced: What I call attraction may possibly be produced by impulse, or in some other manner unknown to us; by attraction I would here be understood to mean only in general a certain kind of force whereby bodies mutually tend towards each other, whatever cause that quality may be ascribed to; for, we must first necessarily know by *Phænomena* of nature, what bodies mutually attract each other, and what are the laws and properties of that attraction, before we can properly enquire by what efficient cause that attraction is produced.' And a little after, he mentions the same attractions as forces, which by the *Phænomena* appear to have a being in nature, tho' their causes be not yet known; and he distinguishes them from occult qualities, which are supposed to flow from the specific forms of things: And in the *Scholium* at the end of his *Principia*, after mentioning the properties of gravity, he adds; 'but the reason of these proper-

ties of gravity I could not deduce from the *Phænomena*, and I do not devise hypotheses; for, whatever is not deduced from *Phænomena* is to be called an hypothesis; and hypotheses either metaphysical or physical, or of occult qualities, or mechanical, have no place in experimental philosophy. — It is sufficient that gravity really exists, and acts according to the laws I have explained, and that it solves all the motions of the celestial bodies and of our sea. And after all this, it is surprising that Mr. *Newton* should be reflected upon for not explaining the causes of gravity, and other attractions by hypotheses, as if it were a crime to content himself with certainties, and let uncertainties alone; and yet the editors of the *Acta Erudit.* for March 1714. p. 141, 142. have told the world, that Mr. *Newton* denies, that the cause of gravity is mechanical; and that if the spirit or agent, by which electrical attraction is performed, be not the æther or subtile matter of *Des Cartes*, it is less valuable than an hypothesis, and, perhaps may be the hylarchic principle of Dr. *Henry More*: And M. *Leibnitz*, in his *Theodicee*, and in his letters to *Hartsoeker* and elsewhere, has accused Mr. *Newton* of making gravity a natural or essential property of bodies, and even an occult quality, and at length a miracle; and by this cavilling they would persuade the *Germans*, that Mr. *Newton* wants Judgment, and was incapable of inventing the infinitesimal method.

It must be allowed, that these two gentlemen, Mr. *Newton* and M. *Leibnitz*, differ very much in philosophy; the former proceeds upon the evidence, arising from experiments and *Phænomena*, and stops where such evidence is wanting; the other is taken up with hypotheses, and propounds them, not to be examined by experiments, but to be believed without examination: The one for want of experiments to decide the question, does not affirm, whether the cause of gravity be mechanical or not; the other pronounces, that it is a perpetual miracle, if it be not mechanical: The one, by way of enquiry, attributes it to the power of the creator, that the minutest particles of matter are hard; the other attributes the hardness of matter to conspiring motions, and calls it a perpetual miracle, if the cause of this hardness be other than mechanical: The one doth not attempt to affirm, that animal motion in man is purely mechanical; the other teaches, that it is purely mechanical, the soul or mind (according to the hypothesis of an *Harmonia præstabilita*, or pre-established harmony) never acting upon the body so as to alter or influence its motions: The one teaches, that God (in whom we live,

live, move and have our being) is omnipresent, but not as a soul of the world; the other that he is not the soul of the world, but an *Intelligentia supramundana*, an intelligence above the bounds of the world; whence it should seem to follow, that he can do nothing within the bounds of the world, unless by an incredible miracle: The one teaches, that philosophers are to argue from *Phænomena* and experiments to their causes, and thence to the causes of those causes, and so on, till we come to the first cause; the other, that all the actions of the first cause are miracles, and all the laws impressed on nature by the will of God, perpetual miracles and occult qualities; and therefore not to be considered in philosophy; but must the constant and universal laws of nature, if derived from the power of God, or the action of a cause not yet known to us, be called miracles and occult qualities, that is to say, prodigies and absurdities? Must all the arguments for a God, taken from the *Phænomena* of nature, be exploded by new hard names? And must experimental philosophy be rejected as superstitious and absurd, because it asserts nothing more than can be proved by experiments; and that we cannot yet prove by experiments, that all the *Phænomena* in nature can be solved by mere mechanical causes? Certainly these things deserve to be better considered.

A new Star in the Neck of the Swan; by M. Gottfried Kirch. Phil. Trans. N° 343. p. 226. Translated from the Latin.

THO' various changes happen among the fixed stars, as to their apparent magnitude, yet none was more surprising than that which *Fabricius* observed in the whale's neck in 1596; tho' at first it was taken for a new star, that had never existed before, and after disappearing was to return no more; yet now it has been sufficiently confirmed by experience, that it still exists, and that it doubtless existed from the beginning of the world in that very place, where it now is: What is surprising in this star is, that it appears every year of a different magnitude, and commonly at certain times it cannot be distinctly seen by the naked eye; for which reason *Hevelius* called it *Stella mira*, or the wonderful star.

M. Kirch likewise observed another star like this in the swan's neck, but much smaller, and seen every year for a shorter space of time; whence it is not surprising, that it was so long unknown; nay, it was lucky, that it appeared at that time, and that it was seen in its greatest magnitude, when *Bayer* was observing and deli-

delineating the stars in the swan, having represented it by χ , and reckoned it amongst the fixed stars of the fifth magnitude, that constantly appear; and at the same time he likewise found the above-mentioned star in the whale's neck, to be of the fourth magnitude, and represented it by α , taking it for a fixed star that constantly appears.

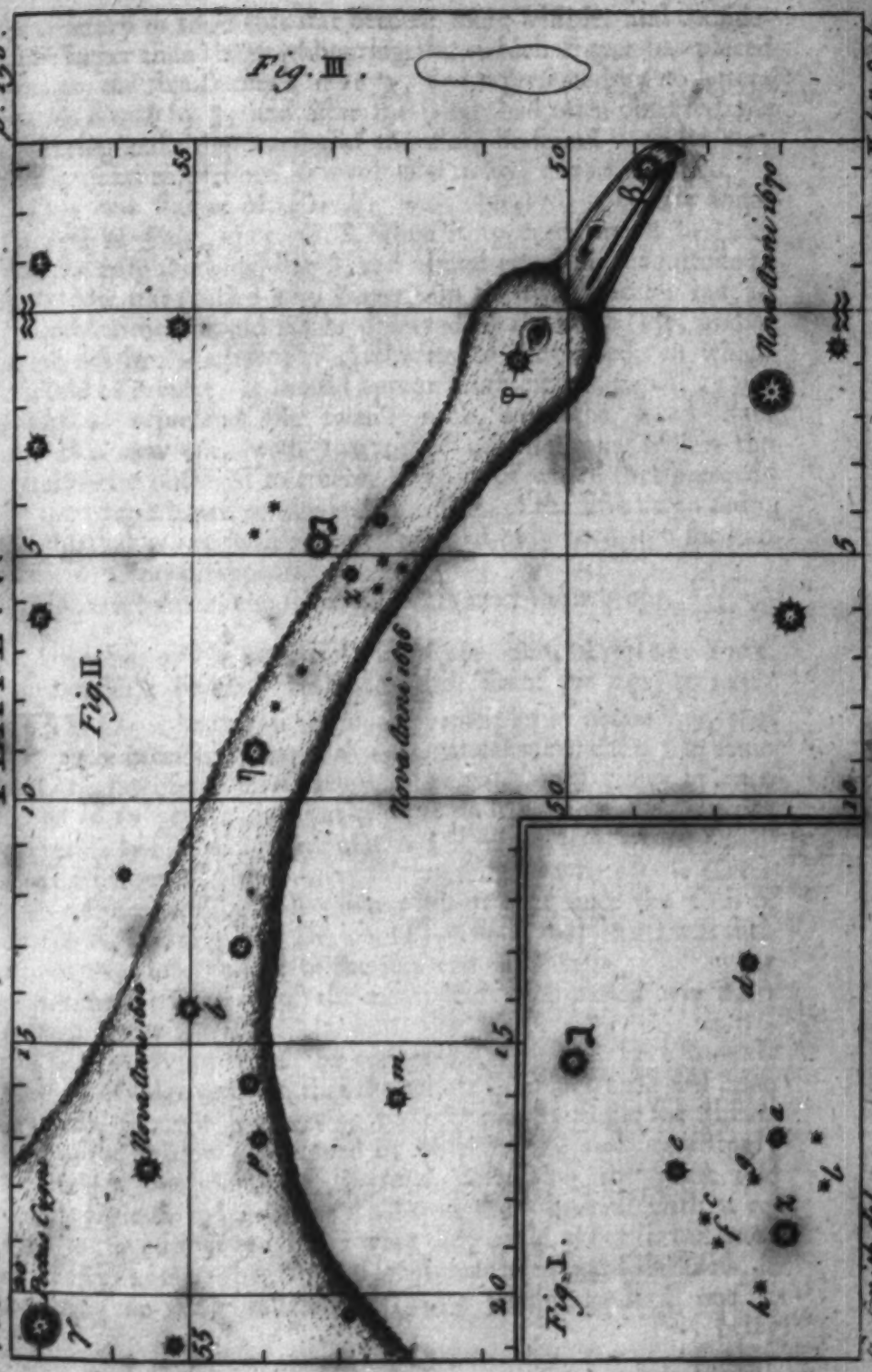
The star next the head of the swan, observed in 1670, and 1671, by *Hevelius*, gave *M. Kirch* the opportunity of finding the mutable appearance of the star χ in the swan's neck; for, when he was in hopes, that the same star would appear frequently in the same manner as that other in the whale's neck, which after its first disappearing had appeared again presently after to *Hevelius*, he sought for it on the first and sixth of *July*, 1686. O. S. both clear nights, but did not find it; but rather found, that that star of the fifth magnitude in the swan's neck, represented by *Bayer* by χ was wanting; yet on the ninth of *October*, O. S. he saw it very plainly with his naked eye; and because he was apt to think, that it would again disappear to the naked eye; he delineated some little stars round it, by means of a two foot telescope, that by comparing these with it, he might compute its magnitude, when it should decrease, as represented by Fig. 1. Plate V. He likewise found, that that star had gradually decreased, till he could not see it thro' an eight foot telescope; tho' he could still observe that other star in the whale's neck thro' a four foot telescope: From that time he looked for that star for several nights together, but to no purpose; but at length he found it again on the sixth of *August* 1687. O. S. with an eight foot telescope, but exceeding small; and from that time he found it daily increase more and more; and it happened, that on the 23d of *October*, O. S. it appeared again for the first time to the naked eye, tho' still exceeding small: On the second of *November* O. S. it was very visible, and even after the 26th of *November* O. S. tho' this last day it was decreasing again; afterwards it could not be seen but thro' a telescope, and at length it became so small, that he could not observe it with an eight foot telescope; and thus *M. Kirch* found at this time, that from one disappearing to another, there was about a year, a month and a week; the following observations did likewise shew, that this star observed a pretty regular period in its appearing, tho' not of an equal magnitude at any particular time thereof; nay it sometimes happened, that it was quite invisible to the naked eye, tho' it could be seen thro' a telescope, and had come to its greatest magnitude; as in the latter end of 1688, and beginning of 1689; on the

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PLATE V.

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P. 150.

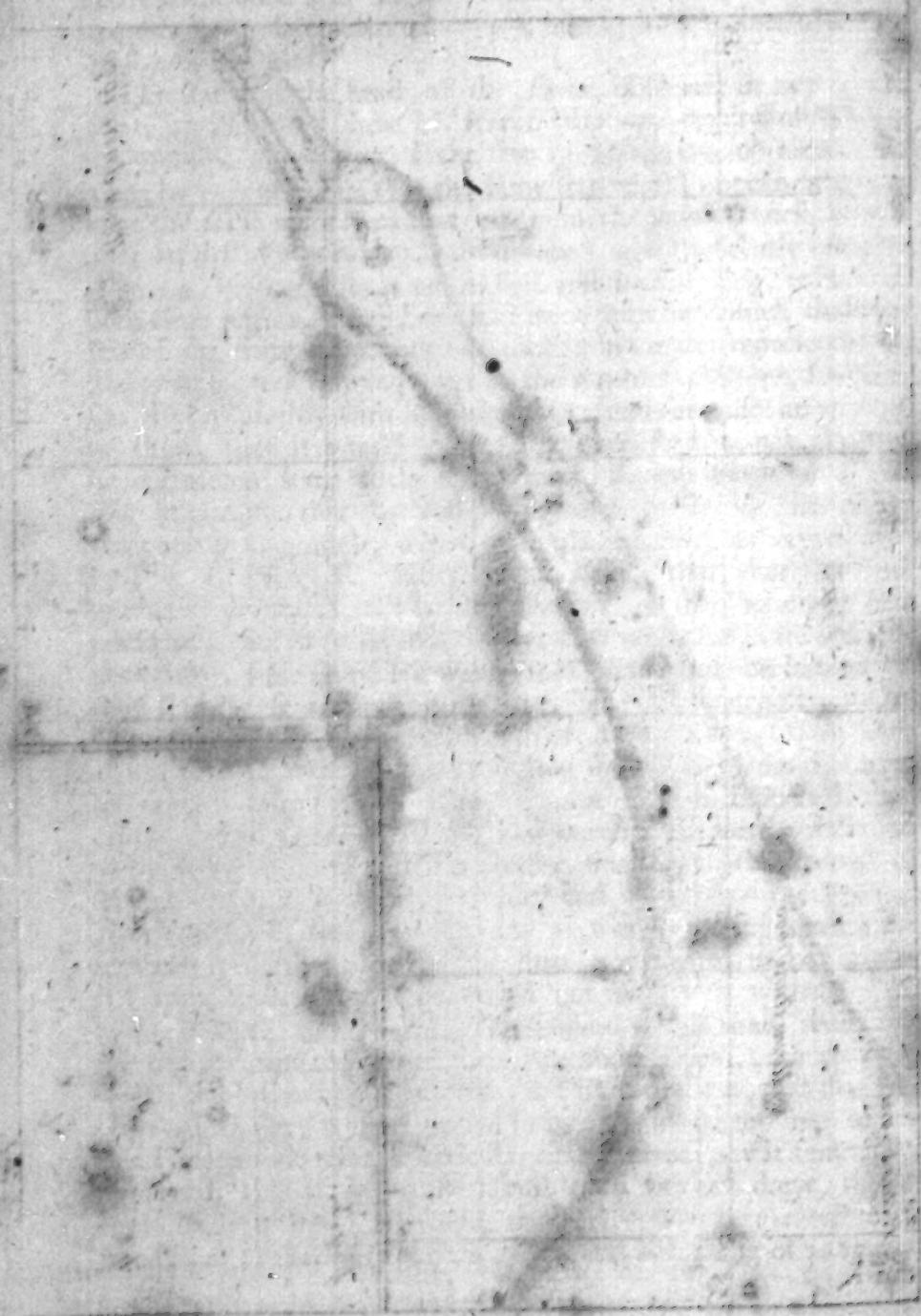


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the contrary in 1690 this star became more visible, and considerably larger than the neighbouring star, which *Bayer* has placed without the swan's neck near χ , and represented by no letter; but *M. Kirch* by β ; and after the latter had often observed the appearing and disappearing of this star, he found it to be very regular, and to perform its revolution in 404 days and a half.

This new star of *M. Kirch's* was observed at *London* about the 15th of *July*, 1715. O. S. when it appeared much brighter than the neighbouring star β , and almost equal in magnitude to the middle star (called η by *Bayer*) in the swan's neck; but in a month's time it could not be observed by the naked eye, and at length not thro' a telescope: According to the period, in which it is said to revolve, it should appear brightest in *August*, 1715.

Fig. 2. represents the swan's neck, with the fixed stars next this new one, with two other new stars that within the century were observed to emerge near it, of which that preceding the swan's breast continues still visible, of the fifth magnitude, but that below the swan's head, that had only been seen for two years, hitherto disappears.

Fig. 1. represents the telescopic stars next the new one.

Observations on the total Eclipse of the Sun, April 22. 1715. at London; by Dr. Halley. Phil. Transf. N° 343. p. 245.

TH O' it be certain from the principles of astronomy, that there necessarily happens a central eclipse of the sun in some part or other of the terraqueous globe, about 28 times in each period of 18 years; and that of these no less than eight pass over the parallel of *London*, three of which eight are total with continuance; yet from the great variety of the elements whereof the *Calculus* of eclipses consists, it has so happened, that since the 20th of *March* A. C. 1140, the Dr. could not find, that there was such a thing as a total eclipse of the sun seen at *London*, tho' in the mean time the shadow of the moon had often passed over other parts of *Great Britain*.

The Dr. having found, by comparing what had been formerly observ'd of solar eclipses, that the whole shadow would fall upon *England*, thought it a very proper opportunity to get the dimensions of the shadow ascertained by observation; and accordingly he caus'd a small map of *England*, describing the track and bounds thereof, to be dispers'd all over the kingdom, with a request to the curious to observe what they could about it, but more especially to note the time of continuance of total darkness, as requiring no other instrument than a pendulum-clock, and as being

being determinable with the utmost exactness, by reason of the momentaneous occultation and emersion of the luminous edge of the sun, whose least part makes day: Nor did this advertisement fail of the desir'd effect, for, the heavens having prov'd generally favourable, there were receiv'd from so many places such good accounts, that they fully answer'd all expectations, and are sufficient to establish several of the elements of the *Calculus* of eclipses, so as for the future we may more securely rely on our predictions; tho' it must be granted, that in this, astronomy had lost no credit.

The day of the eclipse approaching, the Dr. receiv'd the orders of the *Royal Society* to provide for the observation to be made at their house in *Crane-Court*; and accordingly he procur'd a quadrant of near 30 inches radius, exceedingly well fix'd with telescopic sights, and mov'd with screws, so as to follow the sun with great nicety; as also a very good pendulum clock well adjusted to the mean time, and several telescopes wherewith to accommodate the greater number of observers.

In order to examine both clock and quadrant, the Dr. on the 20th of *April*, observ'd the distance of the upper limb of the sun from the zenith $36^{\circ}.16'$. and the next day $35^{\circ}.58'$. by which it appear'd, that the distances from the zenith taken by this quadrant, ought to be increas'd by about a minute; and that allowance being made, by several observations, taken before and after noon on the said 21st day, the clock was found to answer the apparent time or hour of the sun with sufficient exactness, as not going above $10''$. too fast. The next day *April* 22. just before the eclipse began, he took three distances of the sun from the zenith, viz. at $7^h 42' 52''$ a. m. the correct distance of the sun's center a vertex was $62^{\circ} 1' 40''$. At $7^h 45' 48''$ it was $61^{\circ} 34' 40''$ and again at $7^h 48' 55''$ it was $61^{\circ} 6' 40''$, which, with the given declination of the sun and latitude of the place, shew the true times respectively to have been $7^h 42' 38''$, $7^h 45' 35''$, and $7^h 48' 39''$. All concurring, that the clock was only $14''$ too fast, and had scarce gain'd any thing sensible in a day's time; so that it might be entirely depended upon during the continuance of the eclipse.

Having computed, that the eclipse would begin at $8^h 7'$ the Dr. attended soon after 8^h . with a very good telescope of about 6 foot, without stirring his eye from that part of the sun at which the eclipse was to begin; and at $8^h 6' 20''$ by the clock, he began to perceive a small depression made in the sun's western limb, which immediately became more conspicuous, so that the Dr.

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concluded the just beginning not to have been above five seconds sooner, that is, exactly at $8^h 6' 00''$ correct time: From this time the eclipse advanced, and by nine o'clock was about 10 digits, when the face and colour of the sky began to change from a perfect serene azure blue, to a more dusky livid colour, having an eye of purple intermix'd, and grew darker and darker till the total immersion of the sun, which happened at $9^h 9' 17''$ by the clock, or $9^h 9' 3''$ true time; this moment was determinable with great nicety, the sun's light being extinguish'd at once; and yet that of the emerfion was more so; for, the sun came out in an instant with a surprising lustre, and in a moment restor'd the day, viz. at $9^h 12' 26''$ true time; after he had been totally obscur'd for $3' 25''$ of time; and as near as the Dr. could estimate the points on the moon's limb, where the last particle of the sun vanish'd, it was about the middle of the south east quadrant of her limb, or about 45 degrees from her nadir to the left hand; and the first emerfion was about 10 degrees below the horizontal line thro' the moon's center on the west side; and at 14 minutes past nine correct time, the Dr. judged the hours of the eclipse to have been exactly perpendicular, and consequently, the centers of the sun and moon to be in equal altitude.

It was universally remark'd, that when the last part of the sun remained on his east side, he grew very faint, and was easily supportable by the naked eye, even thro' the telescope, for above a minute of time before the total darkness; whereas, on the contrary, the eye could not endure the splendor of the emerging beams in the telescope from the first moment; to this, perhaps, two causes contributed; the one, that the pupil of the eye did necessarily dilate itself during the darkness, which before had been much contracted by looking on the sun; the other, that the eastern parts of the moon, having been heated with a day near as long as 30 of ours, could not fail of having that part of its atmosphere replete with vapours, rais'd by the so long continu'd action of the sun, and consequently, it was more dense near the moon's surface, and more capable of obstructing the lustre of the sun's beams; whereas, at the same time the western edge of the moon had suffer'd as long a night, during which there might fall in dews all the vapours that were rais'd the preceeding long day; and for that reason, that part of its atmosphere might be seen much more pure and transparent; but from whatever cause it proceeded, the thing itself was very manifest, and observ'd by every person.

About two minutes before the total immersion, the remaining part of the sun was reduced to a very fine horn, whose extremities seemed to lose their acuteness, and to become round like stars; and for the space of about a quarter of a minute, a small piece of the southern horn of the eclipse seemed to be cut off from the rest by a good interval, and appeared like an oblong star, rounded at both ends in the form represented by Plate V. Fig. 3. which appearance could proceed from no other cause but the inequalities of the moon's surface, there being some elevated parts thereof near the moon's southern pole, by the interposition of which, part of that exceedingly fine filament of light was intercepted: a few seconds before the sun was entirely hid, there was discover'd round the moon a luminous ring about a digit, or perhaps, a tenth part of the moon's diameter in breadth; it was of a pale whiteness, or rather pearl colour, seeming to the Dr. a little tinged with the colours of the *Iris*, and concentric with the moon, whence he concluded it to be the moon's atmosphere; but the great height thereof far exceeding that of our earth's atmosphere, and the observations of some, who found the breadth of the ring to increase on the west side of the moon, as the emersion approach'd, together with the contrary sentiments of others made the Dr. less positive, especially, in a matter, where, he confesses, he did not give all the attention requisite; whatever it was, this ring appeared much brighter and whiter near the body of the moon than at a distance from it; and its outer circumference, which was not well defined, seemed terminated only by the extreme rarity of the matter it was compos'd of, and in all respects resembled the appearance of an enlightened atmosphere view'd from afar; but whether it belonged to the sun or moon, the Dr. pretends not to determine.

During the whole time of the total eclipse, the Dr. kept his telescope constantly fix'd on the moon, in order to observe what might occur in this uncommon appearance, and he found, that there were constant flashes or coruscations of light, which seemed for a moment to dart out from behind the moon, now here, now there on all sides, but especially on the western side a little before the emersion; and about two or three seconds before it, on the same western side where the sun was just coming out, a long and very narrow streak of a dusky, but strong red light seemed to colour the dark edge of the moon, tho' nothing like it had been seen immediately after the immersion; but this instantly vanish'd upon the first appearance of the sun, as did also the aforesaid luminous ring.

As to the degree of darkness, it was such that one might have expected to have seen many more stars, than the Dr. finds were seen at *London*; the three planets *Jupiter*, *Mercury* and *Venus*, were all that were seen by the gentlemen of the *Society* from the top of their house, where they had a free horizon; and the Dr. did not hear that any one in *London* saw more than *Capella* and *Aldebaran* of the fix'd stars; nor was the light of the ring round the moon capable of effacing the lustre of the stars, for, it was vastly inferior to that of the full moon, and so weak, that he did not observe, that it cast a shadow; but the under parts of the hemisphere, especially in the south east, under the sun, had a crepuscular brightness; and all around, so much of the segment of our atmosphere, as was above the horizon, and without the cone of the moon's shadow, was more or less enlightned by the sun's beams; and its reflexion gave a diffus'd light, which made the air seem hazy, and hinder'd the appearance of the stars; and that this was the real cause thereof, appears by the darkness being more perfect in those places, near which the center of the shadow pass'd, where many more stars were seen, and in some not less than 20, tho' the light of the ring was alike to all.

During the time that the sun recover'd his light, several altitudes were taken, to examine the regularity of the clock's motion; and tho' the sun rose now much lower than at the beginning, yet they all agreed within a very few seconds, that the clock went still a quarter of a minute too fast; and the end of the eclipse approaching, the Dr. attended the moment thereof with all possible care, and concluded the compleat separation of the sun and moon at $10^h 20' 15''$ by the clock, or exactly $10^h 20'$ correct time.

There were present several members of the *Royal Society*, among whom the Lord Chief Justice *Parker* did, at the moment of the emerfion from total darkness, observe the distance of *Jupiter* from the zenith $48^{\circ} 29'$, by which the time thereof is verified; there were also present several foreign gentlemen, and among them M. *Le Chevalier de Louville*, and M. *Monmort*, both members of the *Royal Academy of Sciences* at *Paris*; the former of which came on purpose to observe this eclipse in *England*, and having seen the beginning, applied himself to take digits with his micrometer, and to observe the occultations of three spots seen at that time in the sun as follows.

At	8	28	20	four digits were eclipsed.
	8	32	37	the first and larger spot touch'd the moon.
	8	33	18	the same was entirely hid.
	8	34	08	the first of the two lesser spots was hid.
	8	34	38	the second of them was hid.
	9	36	01	the emersion of the larger spot.
	9	38	26	the emersion of the first lesser spot.
	9	40	25	the emersion of the second lesser spot.
	10	20	04	the end of the eclipse.

And he determined the time of the total darkness $3^{\circ} 22^{\circ}$, or 2° less than by Dr. *Halley's* account.

The heavens were all the while very favourable, and there was little or no wind, and not so much as one cloud interrupted the view from the beginning to the end; but no sooner was the eclipse over, than a great body of clouds hid the sun for several hours after: These observations, having been made with all possible care, are not, it is hoped, far from the truth.

The accounts from other places are as follows; Mr. *Pound* being furnish'd with very accurate instruments, and well skill'd in the matter of observation, and having rectified his clock by several altitudes of the sun, taken both before and after, gave the following account of the principal phenomena observ'd at *Wansted in Essex*.

At	8	6	37	the eclipse first perceiv'd.
	9	9	28	the total immersion.
	9	12	48	the emersion.
	10	20	32	the just end of the eclipse.
	0	3	20	the continuance of total darkness.

The near agreement of this observation with Dr. *Halley's* (the difference being only what is due to the difference of the meridians) makes the Dr. less solicitous for what was observ'd at the *Royal Observatory at Greenwich*, from whence we can only learn, that the duration of the total darkness was $3^{\circ} 11^{\circ}$.

Mr. *Derham*, assisted by Mr. *Molyneux*, &c. made the following observations at *Upminster in Essex*, viz.

H.	'	"	
At	8	7	41 the eclipse began.
	8	33	46 the moon touch'd the larger spot.
	8	34	36 she touch'd the middle spot.
	8	35	41 she touch'd the third spot.
	9	10	38 the total darkness began on a sudden,
			and <i>Aldebaran</i> appear'd.
	9	14	6 the emerſion or end of total darkness.
	0	3	8 continuance of total darkness.
	9	42	41 the third and laſt ſpot diſcover'd.
	10	21	45 the end of the eclipse by a telescope 13
			foot and a half.

And a little before the beginning of the eclipse, he found the larger and preceeding spot to be more northerly than the sun's center 373 and $\frac{1}{2}$ such parts, whereof the sun's diameter was 1647, and that it followed his western limb 0' 43" of time; by which data the situation of that spot is well determined.

Dr. *John Keill*, by reason of clouds, saw nothing distinctly at *Oxford* but the end, which he observed at 10^h 15' 10". As to the total darkness, he could only estimate it by the sudden change of the light of the sky, and reckoned its continuance but 3' 30", which was certainly too little, the center of the shadow having without doubt past very near *Oxford*. Mr. *Cotes*, only observed at *Cambridge* the occultations of the three spots, viz. of the first and greatest at 8^h 34' 11", of the second at 8^h 35' 13", and of the last at 8^h 36' 35": He also observed the end of total darkness at 9^h 14' 37", and the exact end of the eclipse at 10^h 21' 37".

There were several accounts sent from some places, which lay near the track of the center of the shadow, and which might have been very proper to determine the greatest continuance of the darkness, as from *Plymouth*, *Exeter*, *Weymouth*, *Daventry*, *Northampton* and *Lynn Regis*; and all agreed, that the whole sun was obscured at those places full four minutes, and at some of them rather more; but these observers gave no account how they measured this time, and therefore it may well be supposed, they took it in a round number, and perhaps, from pocket minute-watches: What the Dr. thinks may best be relied on for this purpose, are two corresponding observations made, the one at *Barton* near *Kettering* in *Northamptonshire*, where by the observation of Mr. *Bridges* with a good pendulum-clock and all due care,

care, the whole sun was hid no more than $3^{\circ} 53''$; the other was by Mr. *Whitfield*, who observed after the same manner at *King's Walden* in *Hertsfordshire* near *Hitchin*, that the total eclipse continued but $3^{\circ} 32''$: Hence it follows, that the center of the shadow passed near the middle between these two places, which are but 30 geographical miles asunder, and situated nearly at right angles to the way of the shadow; and therefore, that the total darkness, where longest, could last but about $3^{\circ} 57''$, or perhaps a second or two more at *Lynn*, and less at *Plymouth*; the velocity of the progress of the shadow gradually decreasing, and its diameter increasing, as it passed on to the eastward; and this situation of the middle line is confirmed by an observation made at the seat of the Lord *Foley*'s at *Witley*, eight miles beyond *Worcester*, whereby it appears, that the total darkness lasted there $3^{\circ} 15''$. Hence it follows, that *Witley* was about three or four miles farther from the center of the shadow on the north-side than *London* on the south; and *Witley* being by *Ogilby*'s mensurations, 118 measured miles from *London*, it is plain, that the center passed over *Islip*, which is, by the same mensuration 57 such miles on that road, and about 5 miles almost due north from *Oxford*; so that the center of the shadow left *Oxford* but very little upon the right hand; this situation agrees perfectly well with the former between *Barton* and *King's Walden*, and as far as the geography of *England* may be depended on, the Dr. concludes the center to have entered upon it about *Plymouth*, and to have passed over *Exeter*, the *Devizes*, *Islip*, *Buckingham* and *Huntington*, leaving *Oxford* and *Bedford* on the right, and *Lynn* on the left, and to have quitted the coast of *Norfolk* about *Wells* and *Blakeney*.

As to the limits of the shadow, both on the north and south-side, they were got with all the exactness the thing is capable of; and as this was the first eclipse of this kind, that had been observed with the attention the dignity of the phenomenon requires, it is to be hoped, that those which may happen for the future to traverse *Europe*, may not be so little regarded as hitherto they have been: As to the southern limit, where the eclipse ceased to be total on the south side of the sun, an account was received of an observation made at *Norton-cours* about 10 miles on this side *Canterbury* by Dr. *Harris*, assisted by that accurate observer Mr. *Stephen Gray*, by which we learn, that the eclipse began there at $8^h 8^m 55^s$ and ended at $10^h 24^m 47^s$, and that the total darkness continued but about a minute, or rather less, the middle thereof being at $9^h 13^m 52^s$; from this duration

it will follow, that *Norton-court* was but about three or four miles within the shadow; and that it was really so is confirmed by the relation of the inhabitants of *Bocton*, about midway between *Norton-court*, and *Canterbury*, who assured Mr. *Gray*, as he was returning home that same day, that the eclipse was not total there, but, as one of them expressed it, before the sun had quite lost his light on the east-side, he recovered it on the west; and that there was a small light left on the lower part of the sun, that appeared like a star: And from *Cranbrook* in *Kent* Mr. *Tempest* gave an account, that he observed there the sun to be extinguished but for a moment, and instantly to emerge again; so that the limit passed exactly over this town, which is about 38 geographical miles from *London*, and very near the right angle, where the perpendicular from *London* falls on the line of the limit, being 3' $\cos$ of time to the eastward of *London*, in the Lat. of $51^{\circ} 6'$, as near as the Dr. could gather.

How it passed over *Suffex* there were not such authentic accounts, but it was total at *Wadhurst* beyond *Tunbridge-wells*, as also for some short time at *Lewis*; but it was not so at *Brightling*, which place being situated on an eminence that has a commanding prospect, all the country to the northward was seen in darkness, whilst they had there some benefit of a small remainder of the sun.

From these observations we may conclude, that this limit came upon the coast of *England*, about the middle between *Newhaven* and *Brightelmston* in *Suffex*, and passing by *Cranbrook* and *Bocton*, left *Canterbury* about four miles on the right hand, and quitted the coast of *Kent*, not far from *Hern* towards the ancient *Regulbium*, now called *Reculver*; so that it seems scarce one third part of *Kent*, and not so much of *Suffex*, out of all the south coast of *Great Britain*, escaped being involved in this darkness.

The northern limit, having passed over a much greater space, had a greater number of observers, and not less curiously determined than the other: By Mr. *Prosser*'s account, that the eclipse was total at *Haverfordwest* for a minute and a half, whence it follows, that *Haverford* was but about six miles within the limit, and therefore, that it entered on *Pembrokeshire* about the middle of *St. Bride's Bay*, leaving *St. David's* and *Cardigan* on the left, and traversing those 2 counties and *Montgomeryshire*, it entered on *Shropshire*, leaving the town of *Shrewsbury* $1^{\circ} 40'$ in the shadow, as was observed there by Dr. *Hollings*; whereby it appears that *Shrewsbury* was about 8 miles within the limit; thence

it

it proceeded by the east-side of *Cheshire*, leaving *Whitaburch* and *Nantwich* a very little without, and passing by *Congleton* went over the *Peak* of *Derbyshire* into *Torkshire*, and crossed the great northern road between *Pontefract* and *Doncaster*, somewhat nearer the former than the latter; for, by the observations of Mr. *Shelton* at *Darrington*, about two miles on this side *Pontefract*, (in lat. $53^{\circ} 40'$ and long. west from *London* $4^{\circ} 40'$ of time, as may be concluded from *Norwood's* measure of a degree) the sun at $9^h 11'$ was reduced almost to a point, resembling the planet *Mars* both in colour and size; but whilst he watched for the total eclipse, that point grew bigger, and the darkness diminished, whence he argued the limit to have been but very little more southerly; and afterwards he was informed, that it was just total in *Barnsdale*, three miles south from thence, and that it was so at *Badsworth*, about the same distance from *Darrington*. Mr. *Daubuz* had a certain account from that place, that the luminous ring round the moon was seen there, which was no where visible but while the eclipse was total: From these *Data* we may safely determine the remainder of this track, and that the edge of the shadow, having past over the rest of *Torkshire*, went off to sea about *Flamborough* head: So that of the 40 counties into which *England* is subdivided, only the 3 more northerly had not the sun entirely hid from them; and 6 others escaped but in part, viz. *Shropshire*, *Cheshire* and *Torkshire*, and the extreme part of *Derbyshire* on the north, and *Kent* and *Sussex* on the south, all the rest of the kingdom suffering more or less an interval of total darkness.

The Dr. considering not this eclipse as universal, but only as it related to *England*, thinks it sufficient to observe, that the shadow came out of the *Atlantic* ocean, passing over the *Azores*; and that the southern limit thereof reach'd the isle of *Ushant*, and the north-west coasts of *Brittany* between *Brest* and *Morlaix*, and dividing the islands of *Guernsey* and *Jersey*, just touch'd upon the promontory of *Normandy*, call'd *Cape la Hague*; and that after it had quitted *England* and traversed the *German* ocean, it fell on *Jutland* on the southside, and *Norway* on the north, and thence proceeded to the eastward over *Sweden*, *Finland*, &c.

It now remains to consider the figure, position, direction, velocity and magnitude of the shadow as it passed over *England*: And first as to the figure, it is obvious, that the shadow of the moon being a cone, and the earth's surface sufficiently spherical, the apparent shadow on the earth will be the common intersection of a cone and sphere, which is a figure, hitherto little considered

by geometers, and not being *in plano* is not to be exactly described, but in the spherical or conical surface: How to find the points of this curve in all cases is taught by P. Courcier, in a very scarce *Latin* book printed at *Dijon* in *Burgundy*, and published at *Paris*, in 1663; nor has any other author handled the same subject after him, tho' both capable and worthy of farther improvement; by what he there delivers, prop. 11, 12. lib. 1. it will be easily understood, that the convexity of so small a part of the earth's surface as the shadow commonly possesses, can produce but an inconsiderable effect; so that without sensible error we may take it for a plane, and the section for a true *Apollonian* ellipsis, whose transverse axis, by reason of the smallness of the angle of the cone, will be to its conjugate nearly as radius to the sine of the sun's altitude at its center, especially, if he be considerably elevated; but when he is near the horizon, it will be necessary to have regard to the true figure, by reason of the great length to which the transverse axis is extended, and particularly when the shadow is entering upon, or leaving the earth's disk.

As to the position of the axis of the shadow, it is manifest, that it must always lie in the plane of a great circle of the earth, passing thro' the axis of the cone of the shadow; and therefore, it will be only requisite to obtain the azimuth and altitude of the sun at the place where the center of the shadow is at any time found to determine the situation of the axis and species of the ellipsis required: Thus the middle of the eclipse at *London* having been observed at 9h. 10' 45" by the given latitude and declination we find his azimuth about $59^{\circ}.00.$ and altitude $40^{\circ}.46'.$ that is, just 40 degrees high at the center of the shadow; wherefore, the transverse axis of the ellipsis was to its conjugate very nearly as radius to the sine of 40° , or as 1000 to 643 *proximè*, and made an angle of 59° , or a very little more, with the meridian passing at that time thro' the center of the shadow.

Next as to the direction and velocity of the motion, with which the center of the shadow passed over *England*; it is to be observed, that the shadow passes in a very compound curve, which in the former is not *in plano*, and only describable on the surface of the sphere; nor is its motion equable, but compounded of a great many elements, producing a considerable variety; but for so small a part of the curve as went over *England*, it may be reckoned a right line, with more exactness than we usually find in most of our geographical charts; and the like

may be said as to the velocity, which, tho' in the present instance it was continually decreasing, may, for so short a time, be supposed to have been the same without sensible error.

By a careful calculation the Dr. determined the velocity of the motion, at the time of the middle of the eclipse at *London*, to have been 29 geographical miles in a minute of time *quam proxime*, and that its way made an angle of $32^{\circ} 45'$ with the meridian towards the eastward of the north; wherefore the said way made an angle with the axis of the ellipsis of $68^{\circ} 15'$; and the greatest duration of total darkness having been $3' 57''$, as was shewn above, it will follow, that that diameter of the elliptic figure, according to which the shadow passed, was no less than 114 geographical miles and a half: And from the elements of conics, it is easy to be proved, that supposing the figure of the shadow a true ellipsis, whose axes are as the radius to the sine of 40 degrees, the greater axis would be 171 geographical miles, and the lesser 110, and the nearest distance between the limits, supposed parallel, would be 164 such miles; and this length of the axis of the shadow, derived purely from the continuance of the total darkness, is fully confirmed by the observed distance of the parallel limits, the one passing by *Badsworth* in *Yorkshire*, the other by *Cranbrook* in *Kent*; for, by the two lat. $53^{\circ} 37'$ and $51^{\circ} 6'$ with the difference of long. $7'$ and $40''$ of time, or $1^{\circ} 55'$, the distance of these two places is given, *viz.* $166\frac{1}{2}$ geographical miles, with the mean angle of position 25° from the north westwards; wherefore, this arch makes an angle with the track of the shadow of 77° and $\frac{1}{4}$, and hence the nearest distance of the parallels becomes 163 such miles, which by the other way was found 164: If, therefore, we conclude the axis of the shadow, when the sun was just 40° high, to have extended over $2^{\circ} 50'$ of a great circle, we may safely determine the difference of the sun and moon's diameters at this time; for, the difference of the horizontal parallaxes of the sun and moon being found to be $60' 38''$, the difference of the parallaxes in altitude at both ends of the axis, will be found to be $1' 56''$, and by so much did the diameter of the moon, when 40 degrees high, exceed that of the sun; hence the horizontal diameter of the moon in this anomaly is found $33' 27''$ which may serve for a rule in all other cases.

There was a chill and damp which attended the darkness of this eclipse, of which most spectators were sensible; and a general consternation appeared in all sorts of animals, as birds, beasts and fishes, upon the extinction of the sun, and we ourselves could not behold it without some sense of horror.

Lastly,

Lastly, the Dr. here adds the following synopsis of such observations, as came to his hands.

Place	Observer	Beginn. h. ' "	Immerf. h. ' "	Emerf. h. ' "	Tot. ' "	End h. ' "
Barton	M. Bridges				3. 53	
Bell-bar.	M. Jones	8. 6. 25	9. 9. 45	9. 13. 27	3. 42	
Broadway			8. 47. 00	8. 49. 30	2. 30	10. 21. 57
Carmarth				9. 14. 37		10. 24. 30
Cambridge	M. Cotes					
Canterbury	M. Gray	8. 10. 00				
Chester	M. Ward	7. 56. 40				10. 6. 35
Crew	M. Wright		9. 2. 8		2. 00	10. 9. 00
Dublin	L. Arch Bish.	7. 42. 11				9. 49. 40
Dublin	M. Hawkins	7. 41. 30				9. 48. 45
Exon	L. Bishop		8. 55. 0	8. 59. 0	4. 00	10. 0. 00
Exon	M. Hudson	7. 47. 30			3. 30	10. 0. 30
Greenwich	M. Flamstead				3. 11	
King's Wald	M. Whitfide				3. 52	
Llanidan	M. Rowland	7. 52. 30				
Anglesey						
London	R. Society	8. 6. 00	9. 9. 3	9. 12. 26	3. 23	10. 20. 00
Northampt.	M. Hawkins		9. 5. 22	9. 9. 24	4. 2	10. 15. 35
Norton-cou.	D. Harris	8. 8. 55	9. 13. 23	9. 14. 22	0. 59	10. 24. 47
Oxon	D. Keill				3. 30	10. 15. 10
Paris	R. Academy	8. 11. 00				10. 28. 00
Plymouth	M. Heines	7. 41. 00	8. 45. 30	8. 50. 00	4. 30	9. 54. 30
Portchester	C. Chandler		9. 2. 25	9. 6. 15	3. 50	
Salop	D. Hollings	7. 58. 0			1. 40	10. 6. 00
Upminster	M. Derham	8. 7. 41	9. 10. 58	9. 14. 6	3. 8	10. 21. 45
Wansted	M. Pound	8. 6. 37	9. 9. 28	9. 12. 48	3. 20	10. 20. 32
Weymouth	M. Hobbs		8. 53. 00	8. 58. 00	4. 00	
Witley.	M. Baxter.	7. 59. 0			3. 15	10. 13. 00

An Account of some Barometrical Experiments for finding the different Elasticity of Air, in several Parts of Switzerland; by Dr. John James Scheuchzer. Phil. Trans. N^o 344. p. 266. Translated from the Latin.

THE tube the Dr. made use of was 32 inches in length, and two lines in diameter, *Paris* measure; the first column in the following tables exhibits the quantity of air left in the tube; the second the height of the mercury above the surface of the stagnant quicksilver; the third the spaces of expanded air; and

and the fourth the descent of the mercury, on account of the air left in the tube.

At Zurich on the sixth of September, 1714, the height of the whole barometer at eight o'clock in the morning, was 26 Paris inches, 4 lines; but at half an hour after nine, it was 26 inches 4 lines and $\frac{1}{2}$

Col. I. inch.	Col. II. inch. lin.		Col. III. inch. lin.		Col. IV. inch. lin.	
3	19	9 twice	12	6 $\frac{1}{2}$ twice	6	7 $\frac{1}{2}$
6	16	8	15	7 $\frac{1}{2}$	9	8 $\frac{1}{2}$
	16	7 $\frac{1}{2}$	15	8	9	9
9						
12	11	11 twice	20	3 twice	14	5 $\frac{1}{2}$
15	9	9 twice	22	6 twice	16	7 $\frac{1}{2}$
18	7	5 $\frac{1}{2}$ }	24	8 $\frac{1}{2}$ }	18	11
	7	6 }	24	8 }	18	10 $\frac{1}{2}$
21	5	3	27	0 twice	21	1 $\frac{1}{2}$
24	3	3	28	11 twice	23	1 $\frac{1}{2}$
27	1	6	30	7 $\frac{1}{2}$ twice	24	10 $\frac{1}{2}$
30	0	4	31	10 $\frac{1}{2}$ twice	26	0

On the 11th of November, at one o'clock in the afternoon, and clear sky on a ridge of mount Liber among the Alps, called Emmenstoen gen aberen, in the jurisdiction of Glaris, the height of the whole barometer 23 inches, 10 lines twice.

Col. I. inch.	Col. II. inch. lin.		Col. III. inch. lin.		Col. IV. inch. lin.	
3	18	7	13	6	5	3
6	15	7 $\frac{1}{2}$	16	4	8	2 $\frac{1}{2}$
9	13	3	18	7	10	7
12	11	1 $\frac{1}{2}$	20	9	12	8
15	9	0	22	9	14	10
18	6	11	25	0	16	11
21	4	11	26	10	18	11
24	3	0	28	10	20	10
27	1	4	30	5	22	6
30	0	2	31	8	23	8

On the 12th of September, at seven o'clock in the morning, and a clear sky, on a high ridge of mount *Liber*, called *Suff* *Suff*, the height of the whole barometer 21 inches, 8 lines.

Col. I.	Col. II.		Col. III.		Col. IV.	
inch.	in.	lin.	in.	lin.	in.	lin.
3	17	6	14	6	4	2
6	14	7	17	3	7	1
9	12	6	19	6	9	2
12	10	5	21	6	11	3
15	8	5	23	6	13	3
18	6	5	25	3	15	3
21	4	7	27	1	17	1
24	2	9 $\frac{1}{2}$	29	0 $\frac{1}{2}$	18	10 $\frac{1}{2}$
27	1	4	30	6	20	4
30	0	2	31	8	21	6

On the 12th of September, at nine o'clock in the morning, and a clear sky, on a high ridge of mount *Liber*, called *Suff* *Suff* *Blattenstock*, the height of the whole barometer was 21 inches 6 lines.

Col. I.	Col. II.		Col. III.		Col. IV.	
inch.	in.	lin.	in.	lin.	in.	lin.
3	17	2 $\frac{1}{2}$	14	6	4	3 $\frac{1}{2}$
6	14	5	17	5	7	1
9	12	4	19	6	9	2
12	10	4 $\frac{1}{2}$	21	5	11	1 $\frac{1}{2}$
15	8	7	23	4 $\frac{1}{2}$	12	11
18	6	7	25	3	14	11
21	4	8	27	3	16	10
24	2	9	29	0	18	9
27	1	3	30	5	20	3
30	0	3	31	6	21	3

On the 14th of September, at 12 o'clock at noon, within the iron mine at *Sarumetum* about 300 paces from its mouth, and the sky clear abroad, the height of the barometer was 24 inches 4 lines, and 24 inches 3 lines.

Col. I. inch.	Col. II in. lin.	Col. III. in. lin.	Col. IV. in. lin.
3	18 9	13 1	5 7
6	15 9	16 1	8 7
9	13 5	18 5	10 11
12	11 3	20 7	13 1
15	9 1	22 9	15 3
18	7 0	24 10	17 4
21	4 11	27 0	19 5
24	3 0	28 10	21 4
27	1 4	30 6	23 0
30	0 3	31 6	24 1

Without this mine in the open air, the Dr. observed the same height of the mercury in the entire barometer, as also in 3 and 9 inches of air left in the tube; but it is to be observed, that the air in the inner parts of the mine, where he made these experiments, was rarified, by a fire that was kindled there the day before, and the place had a moderate warmth like a stove.

It was found by several experiments made before the *Royal Society of London*, that the elastic forces of compressed air are directly as the compressing weights; and by Dr. *Scheuchzer's* observations it is evident, that the same ratio holds very nearly in rarified air; for, tho' there be found some difference, yet it is not so considerable, but that it may easily arise from the inequality of the diameter of the tube; that these experiments may be duly performed, the capacity of the tube must be divided into equal parts, by pouring into it an ounce of mercury only at a time, instead of dividing its length into equal parts.

An Account of an Experiment made to discover the law of the Magnetic Attraction; by Dr. Brook Taylor and Mr. Francis Hauksbee. Phil. Trans. N° 344. p. 294.

DR. Taylor and Mr. Hauksbee made an experiment with the large loadstone belonging to the *Royal Society*, in order to discover the law of the magnetic attraction, of which there was an account

account in a former *Transaction*; since that time, Mr. Hawksbee made another experiment of the same nature with a smaller loadstone, of which he gave an account in *Phil. Transf.* N° 335.

But upon comparing the numbers of that experiment with those of the other, the Dr. found the numbers of the first experiment to be a great deal more regular; wherefore, he concludes that to be the best experiment, of which he gives the following account.

The large loadstone, belonging to the *Royal Society*, was placed in such a manner, as that its two poles lay in the plane of the horizon, and in a line, exactly at right angles with the natural direction of the needle, that was made use of, which was that Dr. Halley had made to observe the variations with; and by means of a carriage contrived for that purpose, the stone was easily moved to and fro, the poles continuing always in the same line; the needle was placed in such a manner, that the center, it play'd upon, was in the same line with the poles of the stone, the north pole being towards the needle; upon measuring the distances from the center of the needle to the extremity of the stone, the variations of the needle from its natural position were found to be as in the following table.

Dist. feet	Variat.	Dist. feet	Variat.	Dist. feet	Variat.
1	81 45	4	16 0	7	3 30
2	58 00	5	9 20	8	2 20
3	30 00	6	5 55	9	1 35

An Account of the Cause of the saltness of the Ocean, and of the several Lakes that emit no Rivers, with a proposal, by means thereof, to discover the Age of the World; by Dr. Halley. Phil. Transf. N° 344. p. 290.

THERE have been several attempts made, and proposals offered, to ascertain from the appearances of nature, what may have been the antiquity of this our terrestrial globe, upon which, by the evidence of sacred writ, mankind has dwelt about 6000 years, or according to the *Septuagint* upwards of 7000; but whereas we are there told, that the formation of man was the last act of the Creator, it is no where revealed in scripture how long the earth had existed before this last creation, nor how long those five days that preceeded it are to be accounted; since we are elsewhere told, that in respect of the Almighty, a thousand

land years are as one day, being equally no part of eternity; nor can it well be conceived, how those days should be understood of natural days, since they are mentioned as measures of time before the creation of the sun, which was not till the fourth day; and it is certain, that *Adam* found the earth, at his first production, fully replenished with all sorts of other animals.

The Dr. proposes an expedient for determining the age of the world by a medium, as he takes it, entirely new, and which in his opinion seems to promise success, tho' the event cannot be judged of till after a long period of time; what suggested this notion to the Dr. was an observation he made, that all the lakes in the world, properly so called, are found to be salt, some more, some less than the main ocean, which in the present case may also be esteemed a lake, since by that term the Dr. means such standing waters as perpetually receive rivers running into them, and have no exit nor evacuation: The number of these lakes, in the known parts of the world, is exceeding small; and indeed, upon enquiry the Dr. cannot determine, whether there are in all any more than four or five; viz. 1. The *Caspian* sea. 2. The *Mare mortuum* or *Lacus Asphaltites*. 3. The lake on which stands the city of *Mexico*, and 4. The lake of *Titicaca* in *Peru*, which, by a channel of about 30 leagues, communicates with a fifth and smaller, called the lake of *Paria*, neither of which have any other exit: Of these the *Caspian*, which is by much the largest, is reported to be somewhat less salt than the ocean: The *Lacus Asphaltites* is so exceedingly salt, that its waters seem fully sated, or scarce capable of dissolving any more; whence in summer time its banks are incrustrated with great quantities of dry salt, of somewhat a more pungent nature than the sea salt, as having a relish of *Sal armoniac*, as the Dr. was informed by a curious gentleman that was upon the place: The lake of *Mexico*, properly speaking, is two lakes, divided by the causeways that lead to the city, which is built in islands in the middle of the lake, undoubtedly for its security; after the model, it is probable, its first founders borrowed from beavers, who build their houses on dams they make in the rivers after that manner: Now, that part of the lake, which is to the northward of the town and causeways, receives a river of a considerable largeness, which being somewhat higher than the other, does with a small fall discharge itself into the southern part, which is lower; of these the lower is found to be salt, but to what degree the Dr. could not hitherto learn, tho' the upper be almost fresh: And the lake of *Titicaca*, being near 80 leagues in circum-

conference, and receiving several considerable fresh rivers, has its waters, according to the accounts of *Herrera* and *Acosta*, so brackish as not to be potable, tho' not quite so salt as that of the ocean; and the like they affirm of that of *Paria*, into which the lake of *Titicaca* does in part discharge itself, and which the Dr. doubts not would, upon enquiry, be found much saltier than it.

Now, the Dr. conceives, that as all these lakes receive rivers, and have no exit or discharge, so it will be necessary, that their waters rise and cover the land, till such time as their surfaces are sufficiently extended, so as to evaporate that water that is poured in by the rivers; and consequently, that lakes must be larger or smaller, according to the quantity of fresh water they receive; but the waters thus exhaled are perfectly fresh, so that the saline particles, that are brought in by the rivers, remain behind, whilst the fresh evaporate: and hence it is evident, that the salt in the lakes will be continually augmented, and the water grow saltier and saltier; but in lakes that have an exit, as the lake of *Genesaret*, otherways called that of *Tiberias*, and the upper lake of *Mexico*, and indeed, in most others, the water continually running off is supplied by new fresh river water, wherein the saline particles are so few as by no means to be perceived.

Now, if this be the true reason of the saltiness of these lakes, it is not improbable, but that the ocean itself is become salt from the same cause; and thereby we are furnished with an argument for estimating the duration of all things, from an observation of the increment of saltiness in their waters; for, if it be observed what quantity of salt is at present contained in a certain weight of the water of the *Caspian* sea, for instance, taken at a certain place, in the driest weather; and after some centuries of years the same weight of water, taken in the same place, and under the same circumstances, be found to contain a sensibly greater quantity of salt than at the time of the first experiment, we may, by the rule of proportion, make an estimate of the whole time wherein the water would acquire its present degree of saltiness: And this argument would be the more conclusive, if by a like experiment a similar increase in the saltiness of the ocean should be observed; for the ocean, as was said above, receives innumerable rivers, all which deposite their saline particles therein, and are again supplied, as the Dr. has shewn elsewhere, by the vapours of the ocean, which rise therefrom in atoms of pure water, without the least admixture of salt; but rivers, in their long passage over the earth, imbibe some of its saline particles,

tho' in so small a quantity as not to be perceived, unless in these their depositories, and after a long tract of time; and if upon repeating the experiment, after another equal number of ages, it shall be found, that the saltness is farther increased with the same increment as before, then what is now proposed as hypothetical would appear little less than demonstrative: But since this argument can be of no use to ourselves, it requiring very great intervals of time to come to the conclusion, it were to be wished, that the ancient *Greek* and *Latin* authors had delivered down to us the degree of the saltness of the sea, as it was about 2000 years ago; for, then it cannot be doubted, but that the difference between what is now found, and what it then was, would become very sensible; and therefore, the Dr. recommends it to the *Royal Society* to procure experiments to be made of the present degree of saltness of the ocean, and of as many of the lakes as can be come at, that they may stand upon record for the benefit of future ages.

If it be objected, that the water of the ocean, and perhaps, of some of these lakes, might at the first beginning of things, in some measure contain salt, so as to disturb the proportionality of the increase of saltness in them, the Dr. will not dispute it; but observes, that such a supposition would by so much contract the age of the world, within the date to be derived from the foregoing argument, which is chiefly intended to refute the ancient notion, some have of late entertained, of the eternity of all things; tho' perhaps, by it the world may be found much older than many have hitherto imagined.

Some Accounts of the great Solar Eclipse on April 22. 1716. in the Morning. Phil. Transl. N^o 345. p. 314.

THE following observations of this solar eclipse have been transmitted from foreign parts: Mr. *John Edens*, being on his voyage to the island of *Teneriff*, observed the eclipse at sea in Lat. (by observation) $34^{\circ} 20'$ and Long. $0^{\text{h}}. 54'$ west from *London*, as he concluded by their distance and position from the island *Forte Ventura*, which they soon after fell in with; it began at $6^{\text{h}}. 49'$, and ended at $8^{\text{h}}. 47'$, this latter he observed very exactly, tho' not quite so accurately the beginning: Had this observer signified what difference of meridians there was found between the place of observation and the west end of *Forte Ventura*, we might, without sensible error, have concluded the true longitude not only of that island, but also of the *Pic* of *Teneriff*, where the *English* and *Dutch* geographers have fix'd their

their first meridian: Mr. *Edens* adds, that the greatest darkness was about $\frac{1}{2}$ of the sun's diameter, or nine digits on the north side.

From *Germany* were transmitted the following accounts.

At *Nuremberg*, the beginning and greatest obscuration could not be seen for clouds, but the end happened at 11h 10' and $\frac{1}{2}$.

At *Hamburg*, the beginning was observ'd at 8h 59', the greatest obscuration at 10h 5' 30", when 11 digits and a half were darkened; the end could not be seen for clouds.

At *Kiel* in *Holstein*, the beginning at 9h 14', the greatest obscuration 10h 19' 20", and the quantity then eclipsed 11 digits 20'; the end was at 11h 29'.

At *Berlin*, the beginning could not be seen for clouds, but the greatest obscuration was at 22' past 10, when 11 digits were eclips'd; the just end was at 11h 34'.

At *Frankfort* on the *Maine*, the eclipse began at 8h. 50' the greatest darkness at 10h. 11'. but perhaps, should be 10h. 1'. and 10 digits 34' were eclips'd; the end was observ'd at 10' past 11.

By whom these observations were made, and with what instruments, we have not hitherto been informed, but hope they may be exact enough to confirm the longitudes of those several places, which are at present reasonably well known.

In the *Nouvelles Littéraires*, p. 404, 405. publish'd at the *Hague*, there is an account of the observation of this eclipse at *Upsal* in *Sweden*, made by Mr. *Jo. Waller* professor of *Mathematics* in that university, who was very careful to observe it accurately, the times being verified by three clocks, perfectly agreeing with each other, and with the sun, but more especially by a quadrant of five foot radius for taking the sun's altitude; by this instrument he determined the height of the pole at *Upsal* $59^{\circ} 51' 54''$. And by the same instrument, a little before the beginning of the eclipse, he found the height of the sun $39^{\circ} 36' 42''$, his clocks then shewing 9h 47' 50", which proves, that they were very near the true time: At 10h 58' 15", the altitude of the sun being $45^{\circ} 17' 29''$ was the beginning of the total darkness, and at 11h 2' 24" was the end thereof, the altitude of the sun being $44^{\circ} 29' 13''$, so that here the duration of the total eclipse was 4' 9", and the middle thereof but a third of a minute after 11. And lastly, the end is said to have happened about 4 minutes before noon, the sun being $45^{\circ} 42' 6''$ high; but in this there is a manifest mistake; for, it makes the time of emersion, or from the

the middle to the end but $33^{\circ} 20''$; whereas, being so near the meridian, it is certain, that this emersion was the greater part of the duration of the whole eclipse, and, consequently, more than an hour; perhaps, the times might be deduced from the altitudes only, and then the mistake might be in supposing the end so much before noon, as it was really after it: However, to prevent all doubts, we have compar'd this observation with what was observ'd of this eclipse at *London*, and find, that in the Lat. of $39^{\circ} 50'$, the place, where the middle of total darkness was at 11h 0' 20'', was near 19 degrees more easterly than *London* (that is, exactly in the meridian of *Dantzick*) and that the eclipse began there at 9h 52' and a half, and ended at 12h 10'; wherefore, the duration could not be 2h 7' 30'', as publish'd in the said *Nouvelles*, the editor not considering, that the beginning could not be seen for clouds, as in the very next words he assures us.

As to the darkness, it was such, that they could scarce distinguish each other; and besides *Jupiter*, *Mercury*, and *Venus*, of the fix'd stars *Cassiopeia*, *Capella*, *Oculus Tauri*, and *Orion* (*Sirius* not being yet risen) were visible.

An Account of a Journey from the Port of Oratava in the Island of Teneriff, to the Top of the Pic in August 1715, with Observations thereon; by Mr. Edens, Phil. Trans. N^o 345. P. 317.

ON Tuesday, August 13, N. S. Mr. Edens and some others set forwards from the port of Oratava at half an hour after 10 in the evening; the night was somewhat cloudy, and the moon in the full at 12 o'clock the night following; at half an hour after 11 we came to the town of Oratava, which is about two miles from the port, where we stopp'd for about half an hour, to get walking staves to assist us in our ascending the steep of the Pic; at one o'clock on Wednesday morning we came to the foot of a very steep ascent, about a mile and a half above the town of Oratava, where it began to clear up; and we saw the Pic with a white cloud covering its top like a cap: At two o'clock we came to a plain in the road which the Spaniards call *Dornajito en el monte verde*, i. e. *The little trough in the green mountain*, so call'd, as Mr. Edens supposes, because a little below this plain, on the right hand as we went, there is a deep hollow, at the upper end of which, there is a wooden spout placed in a rock, thro' which there runs very clear and cool water, which comes from the mountains; and at a descent a little lower than the spout, there is a trough into which the

water

water comes: At three o'clock, after travelling a road, which was sometimes pretty smooth, and at other times very rough, we came to a little wooden cross by the road side on the left hand, which the Spaniards call *la cruz de la solera*; i. e. the cross of the solera; a Solera is a long pole with a hole at each end, which the Spaniards use to draw wood with, by fastening one end to the wood, and the other to the oxen; this cross was made of a piece of a Solera, and for that reason was so call'd; but why it was set up in this place, Mr. Edens could not tell, unless somebody was kill'd thereabout; at this place we also saw the *Pico* before us, and tho' we had come up hill quite from the port, yet it seem'd to us almost as high here, as when we were there, the white cloud still hiding the greatest part of the *Sugar-loaf*.

After riding about half a mile farther, we came to the side of a hill, which was very rough and steep, call'd *Caravela*, where are a great many pine-trees that grow on both sides the road for a great way; one of which, that was close to the road on the right hand as we went, had a large branch growing out, which, with the boughs that were upon it look'd like the fore-part of a ship; and from the resemblance this tree has to a ship, Mr. Edens supposes the place took its name; for *Caravela* signifies an old fashion'd vessel, formerly much us'd in Spain, sharp before, ill shap'd every way, with all the masts inclining forwards; amongst these trees, we saw not a great height in the air, the sulphur discharge itself like a squib, or serpent made of gun-powder, the fire running downwards in a stream, and the smoke ascending upwards from the place where it first took fire; and like this we saw another, whilst we lay under the rocks the next night at *La Stanoba*, part of the way up the *Pico*; but Mr. Edens could not observe, whether either of them gave any report as they were discharged.

At $\frac{1}{2}$ after 4 o'clock we came to the top of this high, round and steep mountain, where grows a tree, called *el pino de la merenda*, i. e. the pine-tree of the afternoon's meal; this is a large tree, and is burnt at the bottom, as having had fires made against it, and in the burnt place there issues out turpentine: These hills are very sandy, and afford burrows for rabbits, which breed there; there is also a great deal of sand found a great way up the *Pico* itself, and not a great way below the foot of the *Sugar-loaf*: At $\frac{1}{2}$ after 5 o'clock we set forwards again, and at half an hour after six came to the *Portillo*, i. e. a branch or gap; we saw the *Pico* about two leagues

leagues and a half before us, still cover'd with a cloud at top; and we were told we were come about two leagues and a half from the port: At half an hour after seven we came to *Las faldas*, i. e. *the Skirts of the Pic*; from whence all the way to *La Stancha*, which is about a quarter of a mile up from the foot of the *Pic*, we rode upon little light stones, for the most part not much bigger than ones fist, and several not much broader than a shilling; and if we kept the beaten track, it was not so deep, but if we turned out of it, the horses went almost over their feet; Mr. *Edens* lighted and made a hole there, in order to find how deep these small stones lay, but he could not find the bottom; whence, he concluded they cover'd the ground for a considerable depth.

There are a great many huge rocks, some of them two miles or thereabouts, from the foot of the *Pic*, which our guide the *Pic-man* told us were cast out from the top of the *Pic*, when it was a *Vulcano*; many of them lay in heaps upwards of threescore yards long; and Mr. *Edens* observ'd, that the farther these rocks lay from the foot of the *Pic* the more they resembled the stones of other common rocks; but the nearer he came to the *Pic*, he found them blacker and more solid; and some of them were glossy like flint, and all exceeding heavy: Those that shone in that manner, he supposes, retained their natural colour; but there were some that looked like slag or dross from a smith's forge; which, undoubtedly, was occasion'd by the extreme heat of the place they came from: Some of these large rocks were thrown out of the *Caldera* or kettle in the top of the *Pic*; and others from a cave or cistern, which is a pretty way up the side of the *Pic*, and by some thought to have no bottom.

At 9 o'clock on *Wednesday* morning we arriv'd at *La Stancha* about a quarter of a mile above the foot of the *Pic* on the east side, where are three or four large, hard, and solid black rocks: There are several mountains to the east of the *Pic* at four or five miles distance, call'd the *Malpeses*, besides one lying a little more to the southward, call'd *la montana de rejada*; all which were formerly *Vulcano's*, tho' not so large as that of the *Pic*, as appears by the rocks and small burnt stones that lie near them, just in the same manner as about the *Pic*: Mr. *Edens* was surpris'd at the exceeding vastness of that huge body; insomuch, that it is impossible to express the manner in which that confus'd heap of rubbish lies; for, it may very well be call'd one of the greatest wonders in the world:

About

About six o'clock at night we saw *Grand Canaria* from *La stancha*, bearing from us E. and by N.

At nine o'clock at night after supper, we endeavoured to get to sleep (laying stones for our pillows and our cloaks for bed-cloaths) but all to no purpose for a great while; some lying pretty near a fire complained of being burnt on one side, and frozen on the other (for, the air was very cutting and sharp) others happened to lie where there were a great many fleas; tho' it be somewhat odd that fleas should be found there, the place being so cold in the night; perhaps, the goats sometimes get under these rocks, and so leave them; and Mr. *Edens* was more apt to think so, because he had found a dead goat in a cave at the very top of the *Pic*; he supposes, that this goat, straggling up thither by chance, was beighted, and feeling the cold, got into this place for heat, where meeting with too great a degree of it, and a very strong sulphureous vapour, it was stifled; for, it was found almost dried to powder.

But to proceed, we fell asleep between 11 and 12 and slept till 1, when waking, our guide told us it was time to prepare for our journey; we immediately rose, and by half an hour after one set out, leaving our horses and some of our men behind: Between *La stancha* and the top of the *Pic* are two very high mountains and the *sugar-loaf*, each of which is almost half a mile's walking; on the first of them the rubbish is smaller, and we were apt to slip back as we ascended; but the uppermost is all composed of hard, loose, rocky, large stones, cast together in a very confused manner; after resting several times we came to the top of the first mountain, where, after being pretty well refreshed, we set forwards again to ascend the second mountain, which is higher than the first, but better to walk on, because of the firmness of the rocks: After we had travelled for about half an hour up the second mountain, we came within sight of the *sugar-loaf*, which before we could not see, by reason of these large interposing hills; after we were arrived at the top of this second mountain, we came to a way, that was almost level, but bearing somewhat up-hill, and about a furlong farther is the foot of the *sugar-loaf*, to which we afterwards came just at three o'clock; the night was clear, and the moon shone very bright, where we were; but below over the sea we could see the clouds, resembling a valley at a vast depth below us; we had a brisk gale, the wind being S. E. and by S. as it was for the most part during our journey: Whilst we sat at the foot of the *sugar-loaf*, we saw the smoke break out in several places; at first like little clouds,
which

which soon vanished, and were succeeded by others from the same or other places.

We set forwards to ascend the last and steepest part of our journey, viz. the *sugar-loaf*, exactly at half an hour past three, and after we had rested twice or thrice, Mr. Edens left the guide and the rest of the company, and ran forwards; and when he was got very nigh the top (which was at $\frac{1}{2}$ after three) two more of the company came up five minutes after, and the rest of the company and the guide came up to the top just at four o'clock.

The shape of the top of the *Pic* is partly oval, the longest diameter lying N. N. W. and S. S. E. and as near as Mr. Edens could conjecture, about 140 yards long, the breadth the other way being about 110; within the top of the *Pic* is a very deep hole, called the *Caldera* or *Kettle*, the deepest part of which lies at the south end; it is, Mr. Edens supposes, 40 yards deep, reckoning from the highest side of the *Pic*, but a good deal shallower, reckoning from the side opposite to *Garachica*; the sides of this *kettle* are very steep, in some places as steep as the descent on the outside of the *sugar-loaf*; at the bottom of this *kettle* lay a great many very large stones; the earth, within side the *kettle*, being rolled up, and put to a candle will burn like sulphur; several places within side the top of the *Pic* burn, as on the outside; and in some places upon turning up the stones one may find very fine sulphur sticking to them; at these holes where the smoke comes out, is a considerable heat: At the N. and by E. side within the top is the cave where the dead goat was found; sometimes the true spirit of sulphur is said to distil therein, tho' it did not drop whilst Mr. Edens was there.

What is said about the difficulty of breathing on the top of this place seems to be false; for we breathed as well as if we had been below; we eat our breakfast there, and continued thereon in all for about two hours and $\frac{1}{2}$: Mr. Edens doubts not but the mercury would have fallen very considerably on this high place, could he have procured a couple of barometers to make trial, but there was no such thing in this island: Before the sun rose Mr. Edens thought the air as cold as ever he felt it in *England* in the sharpest frost; he could scarcely endure his gloves off; there was a great dew all the while he was there till sun rising, tho' the sky looked as clear as possible: A little after sun rising we saw the shadow of the *Pic* upon the sea, reaching over the island of *Gomera*; and the shadow of the upper part, viz. the *sugar-loaf* we saw imprinted like another *Pic* in the sky itself, which was a surprising sight; but the air being cloudy below us,

we saw none of the other islands but *Grand Canaria* and *Gomeræ*.

At six o'clock on *Thursday* morning we came down from the top of the *sugar-loaf*; at seven we came to the cistern of water, which is reported to be without a bottom; this the guide said was false, for about seven or eight years before, when there was a great *Vulcano* in this country, the cave was dry, and he walked all round it, and he said, that the deepest water did not exceed two fathoms: The dimensions of this cave Mr. *Edens* conjectures to be, as follows; its length about 35 yards; breadth 12, and ordinary depth 14 from top to bottom: Upon the farthest side there grows a white substance, which the *Pic-man* said was salt-petre; there was both ice and snow in it, and the ice was of a considerable thickness, covered with water about knee-deep; we let down a bottle by a string for some of the water, in which we put some sugar and drank it, but it was the coldest Mr. *Edens* ever drank in his life, and very clear; a little to the right hand within the cave, the ice was risen up in a heap, in form of a steeple-spire or *sugar-loaf*; and here he thinks, the water comes in.

In our way home we came by a cave three or four miles from the *Pic*, where are a great many skeletons and human bones; and some affirm, that there are giants bones in this cave; but Mr. *Edens* could not tell how many bodies lay there, nor how far the cave might go.

The left Ventricle of the Heart of a prodigious Bigness; by Dr. James Douglass. Phil. Transl. N° 345. p. 326.

THE Dr. opened a young man in *St. Bartholomew's Hospital* in *London*, that died of a palpitation of the heart; its violent beating, and prodigious subsultory motion, for some months before his death, was not only easily felt by laying the hand on the region of the heart, but visible to the naked eye by the rising and falling of the bed-cloaths, that covered the patient; and what was very surprising, sometimes the trembling and throbbing made such a noise in his breast, as could plainly be heard at some distance from his bed-side; this was accompanied with frequent *Deliquium's*, sometimes slow, sometimes swift, and often intermitting.

John Fernelius in his *Pathologia lib. 5. cap. 12.* gives an account of a very uncommon and surprising case of this kind; where he says, the frequent concussion of the heart was so violent and strong, as not only to luxate, but even to break some of

the adjoining ribs: *Franciscus de la Bœ Sylvoius* has a parallel observation in his account of this disease: *Theodorus Kerkringius* in his *Spicilegium Anatomicum*, Obs. 16. gives an account of a woman he opened, whose heart was of a prodigious bigness: And to mention no more, *M. Dionis* at the end of his anatomy gives a large account of a very uncommon case, in which the right auricle of the heart was prodigiously dilated, even to the bigness of the head of a new born child.

In the dissection of this morbid heart the Dr. observed the following remarkable particulars. 1. That the *Pericardium* or *Capsula cordis* was very thick, and firmly adhered, or grew by a fibrous connection to all the outer surface of the heart. 2. Instead of the water, called *Liquor pericardii*, there was only in some places about the basis of the heart a mucilaginous clear substance like a jelly. 3. In the right auricle, when laid open, there was nothing preternatural; the ascending and descending *Cava* opened into the same as usual; the vestige of the *Foramen ovale* with its semicircular *Limbus* was very plain; and the orifice of the coronary vein of the heart was exceeding large, yet its valve was less than usual. 4. In the right ventricle laid open, the *Valvule* called *Tricuspidæ* were of the usual configuration; the sides of this cavity were thin, and full of small fleshy *Columnæ*, as they commonly are, with great variety of furrows and little holes; the three sigmoid or semilunar valves in the mouth of the pulmonary artery, were in a natural state. 5. The left auricle was not much bigger than ordinary; but its muscular appendage, called by *Mr. Comper* the bulb of the pulmonary vein, was exceedingly dilated, and enlarged beyond any thing the Dr. ever saw. 6. The left ventricle, whose capacity in a natural state is always less than the right, was here considerably larger; and had both been injected with any liquor, this had certainly contained three times more than the other. 7. The *Valvule mitrales*, placed at the orifice of this ventricle were much thicker in substance than ordinary, and the two fleshy *columnæ*, called by *Nicolaus Massa*, *duo parvi muscoli*, which send out abundance of small tendons to be inserted into these valves, were proportionably augmented in bulk. 8. The semilunar valves in the mouth of the *Aorta*, were very much preternaturally affected, as would easily appear upon comparing them with those in the orifice of the pulmonary artery, in which they are thin and very broad, so as to be able to shut the cavity of that vessel, and hinder the blood from returning back into the ventricle, and are likewise transparent; but in this they were very thick, contracted

it were, furled up together, and of a whitish colour; and in all appearance, had the person lived longer, they would have turned bony.

After demonstrating this uncommon structure of the heart, the Dr. accounts for the following phenomena; as 1. The palpitation of the heart, which was the chief symptom and complaint of the patient. 2. The preternatural dilatation and enlargement of the left ventricle. As to the first, it is not improbable, but that the firm adhesion of the *Capsula* of the heart to its substance, occasioned that uncommon trembling and throbbing; its free and easy motion being hindered by that thick *involucrum*, which surrounded it so close on each side: Dr. *Lower* in his treatise *de corde humano* gives us such another instance, and explains the palpitation in the same manner: As to the second, viz. the dilatation of the left ventricle and muscular bag of the pulmonary vein, that was altogether owing to the ill configuration of the valves now described; for, as the *Aorta* or great artery arises out of this ventricle, it has three valves, which separating give passage to the blood from the ventricle into the vessel; and in a natural state they shut that passage, and so prevent the blood from recoiling into the same, if it should endeavour to return: But in this case, by reason of its contraction and thickness, not being able to shut the passage, the blood flowed back again into the cavity, which it had gradually enlarged, and dilated to the bigness above-mentioned; besides, the muscular valves not being duly qualified for the performance of their office, the blood recoiled into the auricle, which it had distended in the like manner: This constant regurgitation or reflux of the blood is, besides, sufficient of itself to produce this extraordinary palpitation.

An Account of the several new Stars that have appeared within these 150 Years, and of the return of that in Collo cygni, and of its Continuance observed in 1715. Phil. Trans. N° 346. P. 354.

WITHIN the space of 150 years last past, there have been more discoveries made of changes among the fixed stars, than in all antiquity before; and tho' it be said, that *Hipparchus*, on occasion of a new star that appeared in his time, was induced to number the stars, and make the first catalogue of them, which was, in the opinion of *Pliny*, *res vel Deo improba*; yet neither he nor any of the ancients have left us the place of that new star, to compare with those seen more latterly; one of

which might, perhaps, be the very same with it, re-appearing after a long period of years: Now, tho' several authors have severally described those seen nearer our times; it may not, perhaps, be improper to give a short recapitulation of what was principally remarkable in each of them, with the times of their first appearance, as far as can be collected.

And first, that star in the chair of *Cassiopeia* was not seen by *Cornelius Gemma* on the 8. of *November* 1572, who says, that that night he observed that part of the heavens in a very clear sky, and saw it not; but that the next night, viz. *November* 9. it appeared with a splendour surpassing all the fixed stars, and scarce less bright than *Venus*: This was not seen by *Tycho Brahe* before the 11. of the same month; but from that time he assures us, it gradually decreased and died away, so as in *March* 1574, after 16 months, to be no longer visible; and at this day no signs of it remain; its place in the sphere of the fixed stars, by the accurate observations of the same *Tycho*, was $0^{\circ} 9' 17''$ from the first star of *Aries*, with $53^{\circ} 45'$ N. Lat.

Such another star was seen and observed by the scholars of *Kepler* to begin to appear on *September* 30. 1604. O. S. which was not to be seen the day before; but it broke out at once with a lustre surpassing that of *Jupiter*; and like the former, it died away gradually, and in about much the same time disappeared totally, there remaining no vestiges thereof in *January* 1604: This was near the ecliptic, following the right leg of *Serpentarius*; and by the observations of *Kepler* and others was in $7^{\circ} 20' 0''$ from the first star of *Aries*, with $1^{\circ} 56'$ N. Lat. these two seem to be of a distinct species from the rest, and nothing like them has appeared since.

But between them, viz. in 1596 we have the first account of the surprising star in *Collo ceti*, seen by *David Fabricius* on the 3. of *August*, O. S. as bright as a star of the third magnitude, which has been since found to appear and disappear periodically; its period being precisely enough 7 revolutions in 6 years, tho' it return not always with the same lustre; nor is it ever totally extinguished, but may at all times be seen with a six foot telescope: This was singular in its kind, till that in *Collo cygni* was discovered; it preceds the first star of *Aries* $1^{\circ} 40'$ with $15^{\circ} 57'$ S. Lat.

Another new star was first observed by *William Janssonius* in 1600 in *pectore*, or rather in *eductione Colli cygni*, not exceeding a star of the third magnitude; this, after continuing some years, became at length so small as to be thought by some to disappear entirely;

entirely; but in 1657, 58 and 59, it again arose to the third magnitude, tho' it soon after decayed gradually to a star of the fifth or sixth magnitude; and at this day is to be seen as such in $9^{\circ} 18' 38''$ from the first star of *Aries*, with $55^{\circ} 29' N.$ Lat.

A fifth new star was first seen and observed by *Hevelius* in 1670 on *July 15*. O. S. as a star of the third magnitude, but by the beginning of *October* was scarce to be perceived by the naked eye: In *April* following it was again as bright as before, or rather larger than one of the third magnitude, yet it quite disappeared about the middle of *August*: Next year in *March* 1672 it was seen again, but not exceeding a star of the sixth magnitude, since which time it has been no further visible, tho' its return has been frequently sought for; its place is $9^{\circ} 3' 17''$ from the first star of *Aries*, and $47^{\circ} 28' N.$ Lat.

The sixth and last is that described in *Phil. Trans.* N^o 343. discovered by *M. Kirch* in 1686, and its period determined to be 404 days and $\frac{1}{4}$; and tho' it rarely exceed a star of the fifth magnitude, yet it is very regular in its returns, as was found in 1714. since that time it has been watched, as the absence of the moon and the clearness of the weather would permit, to observe the first beginning of its appearance in a six foot telescope, that bearing a very great aperture, discovers the most minute stars; and on *June 15*. 1715, it was first perceived like one of the very least telescopic stars; but in the rest of that month and *July* following it gradually increased, so as to become in *August* visible to the naked eye, and so it continued all the month of *September*; after that time it again died away by degrees, and on the 8. of *December* at night was scarce discernible by the telescope, and as near as could be conjectured, equal to what it was at its first appearance on *June 15*. so that this year it has been seen in all near six months, which is but little less than half its period; and the middle, and consequently, the greatest brightness falls about the 10. of *September*: Such as have a mind to seek for it may expect its first appearance in *July* 1716, and find it nearly in $9^{\circ} 6' 30''$ from the first star of *Aries*, with $52^{\circ} 40' N.$ Lat.

An Account of the Pareira Brava; by Dr. Helvetius. Phil. Trans. N^o 346. p. 365.

DR. *Helvetius* was one of the first that introduced the *Pareira Brava* into *France*, having made a great many successful experiments about it.

The *Pareira Brava* is a root which comes from *Brazil* by the way of *Lisbon*; it is sold at *Paris* for 40 livres a pound; it

is called in *Brasil* the universal medicine, and is made use of there in all kind of distempers: A capuchin monk, who came from thence, told the Dr. that he could not give it a greater character, than by assuring him, that in all their voyages they carried the gospel in one pocket, and the *Pareira Brava* in the other: It restores digestions and so prevents the forming much phlegm and acid crudities in the first passages; it also hinders the serosity of the blood from spreading itself too much upon the parts; for, experience shows us, that it plentifully provokes urine, whence it follows, that it will discharge by the kidneys, the corrosive acidity of the mass of blood; it is also good to break and dilute the pituitous and viscous humours, and it cures suppressions of urine, occasioned by obstructions in the kidneys: Hence one may conclude, that the salts of the *Pareira Brava*, which are moderately volatile, are proper to dissolve or separate the too thick and too close texture of the sulphur of the *Lympha*; in fine, this medicine has a light or gentle bitterness, which corrects the acids of the stomach, and renders them more pure and fine; hence the chyle becomes better digested and more balsamic, and fitter to assimilate itself with the blood, and to preserve therein that degree of division and fluidity, which is necessary for it.

The method of using this root with success, is to reduce it to an impalpable powder, and infuse the weight of a demi-gros thereof, in a pint of boiling water, and let it lie therein all night, and next morning boil it for a moment; then pour the liquor off gently from the powder, and take a demi-setier of it in two cups with a little sugar, as hot as tea, putting into each cup five grains of the said root, reduced to an impalpable powder, which you must stir with a spoon, that none of it may remain at the bottom; you may repeat the same dose about four hours after dinner, but you must not eat any thing for an hour after you have taken it: This medicine does not oblige one to alter the ordinary course of his living, and one may continue the use of it for several months together, in which time one may also at pleasure discontinue it for two or three days together at a time; but the patient should take some gentle purge every fortnight or three weeks during the use of the said medicine.

The Preparation of the Pareira Brava.

Take 11 grains of this root, and put it into a pewter tea pot with boiling water, and so let it infuse all night over warm ashes, or a very gentle fire, and in the morning boil it again, but very gently, till you use it: You must drink it just as you do tea,

tea, and the liquor which comes from this infusion, must not exceed the quantity of five small dishes of tea.

An Account of some large Teeth, dug up in the North of Ireland; by Mr. Francis Nevile; with Remarks thereon, by Dr. Molyneux. Phil. Trans. N° 346. p. 367.

WITHIN eight miles of Belcarbet, at a place called *Magbery*, in part of the bishop of *Killmore*'s lands, upon digging a foundation for a mill, near the side of a small brook, that divides the counties of *Cavan* and *Monaghan*, there were found four teeth, two of a larger, and the other two of a smaller size; one of the largest was the farthest tooth in the under jaw, the other was like it, and belonged to the opposite side; the lesser tooth Mr. *Nevile* takes to be the third or fourth tooth from it, and it had its fellow; these are all that were found, and one of them in a piece of the jaw-bone, which mouldered away, as soon as taken out of the earth; there was also found part of the skull, of a very large size and thickness, but as soon as exposed to the air it likewise mouldered away.

The place, where this monster lay was prepared in the following manner (which inclined Mr. *Nevile* to think it had been buried, or that it had lain there since the deluge) *viz.* it was about four foot under ground, with a little rising above the superficies of the earth, which was a plain under the foot of a hill, and about 30 yards from the brook or thereabouts; the bed on which it lay, had been laid with fern, with that sort of rushes here call'd *sprits*, and with bushes intermixed; under this was a stiff blue clay, on which the teeth and bones were found; over this, there was first a mixture of yellow clay and sand, much of the same colour, under that a fine white sandy clay, which was next the bed; the bed was for the most part a foot thick, and in some places thicker, with a moisture quite thro' it; it lay close and cut much like turf, and would divide into flakes, thicker or thinner as you pleased; and in every layer the seed of the rushes was as fresh as if new pulled, so that it was in the height of seed-time, that these bones were laid there; the branches of the fern, in every layer as we opened them, were very distinguishable, as were the seeds of the rushes and the tops of the boughs; the whole matter smelt very sour, as it was dug, and tracing it, Mr. *Nevile* found it 34 foot long, and about 20 or 22 foot broad.

It will be worth while to consider what sort of animal this might be, whether human or brutal; if human, there is some reason

reason for the interment, and for that preparation of the bed it was laid on; if brutal, it was not worth the trouble; if human, it must have been larger than any giant we read of; if brutal, it could be no other than an elephant, and we do not find, that they ever were the produce of this climate; and considering how long this must have lain there, Mr. *Nevile* does not think the inhabitants then had any curiosity or convenience to bring such into this kingdom; for, he supposes that the best of their ships could not carry one: Then if it be an elephant, or some other animal, which must have a proportion to the teeth, it must have lain there ever since the flood; and if so, then the bed, on which it lay, must be of its own making; whence it will follow, that the flood coming upon him while he lay in his den, he was there drowned, and covered with slime or mud, which since that time is turned into the substance of the earth above-mentioned.

It is to be observed, that there were a great many nut-shells found about the bed, perhaps, these might have been on the bushes, which composed part of the bed: The two large teeth were of equal weight, two pounds three quarters each; the two small teeth were six ounces each; but some of them were wasted, and some of the roots that enter the jaw, were broken off.

Dr. *Molyneux*, after having particularly examined the four teeth above-mentioned, was fully convinced, that they must certainly have been the four grinding teeth in the lower jaw of an elephant; and that the several loose fragments of those large bones found with them, must have been remains of the same animal: This the Dr. takes to be one of the greatest rarities hitherto discovered in *Ireland*.

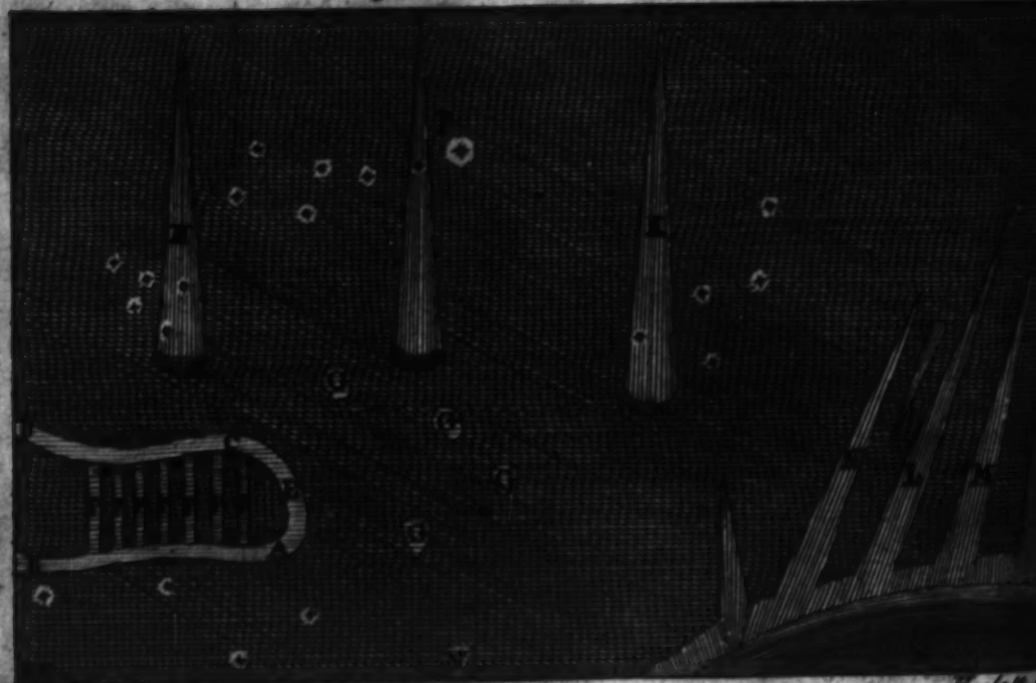
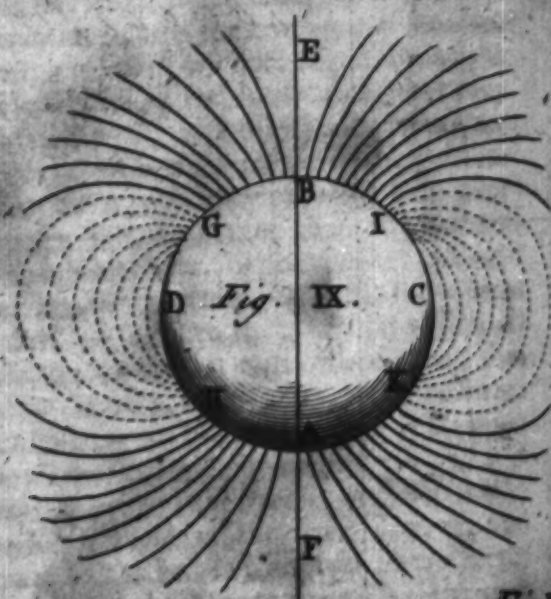
In Plate VI. Fig. 1. A A represents the large grinder of the under-jaw on the right-side, weighing two pounds and three quarters; b, b, b, b, b, b, b, are seven white, rough, indented borders, of an irregular shape, rising about one tenth of an inch higher than the hard, black, shining surface of the tooth; this rough raised work serves for the bruising and grinding the animal's food, viz. the rough grains of rice, leaves, fruits and the boughs of trees; and is of so exceeding hard a contexture as to resemble large knotted threads of white glass, laid on, and closely fastened to the dark superficies of the tooth, and answer to that glassy surface or enamel, with which nature has armed the outside of the teeth of most animals, to prevent their wearing by the constant attrition in chewing their food; c, c, c, c, c, is that part of the tooth which rises above the gums, and even then was distinguished from the rest of the bone, by having its colour of a different shade; d, d, d, d, d, d, d, are several strong fangs or roots

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roots, seemingly united together, whereby the tooth received its sense and nourishment; and tho' so large and ponderous, by these it was kept firmly fixed in the jaw.

As to the mechanism nature followed in framing the teeth of this animal, it is no more than this; whereas in other animals, she has divided that bony substance, wherewith they chew their food, with each its peculiar roots, to secure its articulation in the jaw-bone, she has in this huge animal for the greater strength, firmness and duration of its teeth, and the better to provide for a compleat attrition of the aliment, in order to perfect the digestion so thoroughly, as to sustain the life of this animal for 2 or 300 years (as is commonly supposed in the east) contrived to make the substance of the teeth in their roots below, and in their upper parts above the gums, to unite closely together, and thus coalescing to form a few large massy teeth instead of many small ones; as for instance, in the human body, that is of so much less a size, the number of the teeth (when the whole set is compleat) is 32; whereas in the elephant, the teeth of both the jaws amount in all but to eight, besides its two large tusks, which rather serve for horns for its defence, than teeth to prepare its food; and therefore the Dr. thinks, not so very properly called teeth.

Fig. 2. E E represents the smaller grinder of the lower-jaw on the same side; and its surface covered over with the same white indented work, as above described for grinding the food; f, f, f, are three large roots that kept it firmly fixed in the jaw-bone; this smaller tooth weighed full six ounces.

Fig. 3. G G represents the large grinder of the lower-jaw on the left-side, much of the size, shape and weight with its fellow, represented by Fig. 1. it shews its roots and all its parts, with the rough, protuberant white work on its upper surface, formed after the same manner, and after the same strong model as the former; and, indeed, if one considers it, it is plain, that were not the teeth of this animal made of so large a size, and withal of so massy and firm a substance, it were absolutely impossible they could resist the force, and bear all that pressure wherewith those vast muscles exert themselves, that move the lower-jaw in mastication in such a strong animal.

Fig. 4. H H is the smaller grinder of the lower-jaw on the same side; it is not so compleat as the small tooth represented by Fig. 2. for, some of the root is wanting, and part of its outer grinding surface is broken off at k k, so that it weighs somewhat less; yet what remains shews exactly the same kind of work and shape with the other tooth, that answered it on the right side.

These four teeth fully compleat the set, with which nature has furnished the lower jaw of the elephant, and are answer'd by just as many more, formed after the same manner in the upper jaw, according to Dr. *Moulins*, who dissected the elephant, burnt at *Dublin* in 1681; in p. 40 of its anatomy, speaking of the teeth, he assures us, that there were, besides the tusks, only four teeth in each jaw, two on each side, and that these eight teeth were all *Molares*, so that he had no *Incisores*.

But notwithstanding this, it may, perhaps, be said, that we should not hastily conclude from hence, that the large teeth dug up in *Ireland* must certainly have been the four grinders of an elephant, since they might as well belong to some other huge animal, either terrestrial or marine: As to the surmise of their being human or gigantic, it is so groundless a thought, and so contradictory to comparative anatomy and all natural history, as not to deserve our consideration: To obviate this, Dr. *Molynæus* observes in general; first, that the different kinds of animals, with which the world is stock'd, are not more distinguish'd by the make of any part of their bodies from each other, than by the various shape and disposition of their teeth; and hence it is, we shall not find any two distinct classes of animals, that exactly agree in the same make and arrangement of their teeth: But to be more particular, and make this point so plain, as to admit of no controversy, the Dr. sets down at length the words of two authors, who have describ'd express the teeth of the elephant; the first is Mr. *Blair*, in his *Osteology* of an elephant in *Phil. Trans.* N^o 326, 327, where giving a description of the teeth of this animal, he says; 'Dr. *Moulins* well observes, that they are all *Molares*, being two inches broad in that part with which they grind, and six inches and a half long on the right side, and 5 inches and a half on the left; their surface, tho' flat, is yet very unequal, for, they have a hollowness and then an eminency, placed alternately, running from the right to the left side; and the eminence is surrounded by a rough, protuberant border; there are nine of these hollownesses, and as many eminencies, undulated like the waves of the sea'.

It is remarkable how very exactly all this agreed with the figures of these found in *Ireland*; it is true, those hollownesses and eminencies, which Mr. *Blair* mentions to be nine, do not so nicely hit with the number of those in the above teeth; but this difference proceeds from hence; that Mr. *Blair* here describes the grinders of the upper, whereas, the others are those
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of the lower jaw; tho' such a distinction as this, Dr. *Molyneux* is apt to think, may very well happen even in those of the same jaw in various animals, from some peculiar disposition in one from the other, nay, and perhaps, in the same animal at different times, according as it happens to be older or younger: A little farther, Mr. *Blair* speaking of the teeth of the lower jaw, says; 'the hinder tooth of the right side is four inches, and that on the left five; the one half of their surface, where they begin to appear above the gums, is semi-circular, with the above-mentioned ridges and *fulci*, running transversely, four on the right side, and five on the left; the other half (or tooth Dr. *Molyneux* supposes Mr. *Blair* means) has five of these eminencies where it grinds on the right, and four on the left; each of the four teeth is six inches long, and has six or seven of the above-mentioned eminencies, and as many depressions; these teeth are the most firm, solid and weighty bones of any animal hitherto known'.

The other author, Dr. *Molyneux* produces for the further illustration of this matter, is the laborious and accurate Mr. *Ray*, who, in his *Synopsis Animalium quadrupedum*, when he comes to give a description of the elephant, speaks to the following purpose; 'the mouth is small in proportion to the bulk of this huge animal, and furnished with four grinders in each jaw, or a congeries of grinders; in regard a number of teeth is so wedged into a hard solid bone, as to form with it, and mutually among themselves, one continued body; these teeth in the elephant form on their surface eight or nine undulated parallel lines, and are whiter than the rest of the bone; the whole congeries like single teeth, are inserted by *Gomphosis* into the jaw-bones; and this animal has no *Incisores* at all'.

Upon comparing the particulars of both these accounts with the description and figures given above of the teeth dug up in *Ireland*, and observing how they all agree exactly even, so as to tally together, the Dr. thinks it will amount to no less than demonstration, and that all these ideas were taken from one and the same natural object.

But then, perhaps, it will be asked, what is become of all the rest of teeth, that were in the upper jaw, which being as firm and solid bones as the former, might for the same reason have still remained entire. But since we find it otherwise, it is obvious to imagine a probable conjecture how this might come about; from what Mr. *Neville* mentions, it is plain, that the bed where all these bones were found must once have been the

outer surface of the earth, the green soil producing rushes, fern and nuts; and when this huge animal first fell dead upon this spot, the skull, with all the bones and teeth of the upper-jaw, being the highest parts thereof, might, it is likely, fall in such a position, as to be exposed for some time above the ground, to the injuries of the air and weather, and so moulder away; tho' those of the under-jaw, first coming to the ground, might make themselves a bed, and being covered with the mould remain entire.

But tho' this be allowed, yet still a greater difficulty remains unsolved; viz. how this huge animal, a native of the remote warmer climates of the world, should be deposited in this wild northern island (where *Greeks* or *Romans* never had a footing) so many miles from sea, and distant from those places of the isle, where people might most probably resort.

And to make the difficulty still greater, we must conclude, not only from the dark black colour of the teeth, contracted by their lying long under ground, and the remarkable alteration wrought on their bony substance, which (by mineral steams and exhalations imbibed whilst they lay in the earth) was become more solid, hard and ponderous than it naturally is at first (nay, in some parts plainly petrified) but also from the mouldering of all the other bones, and from the considerable depth of earth, that covered those that were found, that they have lain in that place for several centuries, tho' not with *Mr. Neville* ever since the flood; because we cannot suppose, that the slight texture of vegetable substances, as nuts and the seeds of rushes, could possibly have been preserved so long; but this, at least, may be safely affirmed, that these remains must be cotemporary with some of the remote ages of the world; which carries us so far back into the earliest times, that we can never imagine the rude inhabitants of *Ireland*, or any of their neighbouring countries, masters of so much art or curiosity, as to transport an animal of this large size from those remote countries, where it was bred; which they that now attempt find a work of vast care, trouble and expence, even in this age, wherein navigation is brought to such perfection.

These considerations, grounded on other instances of the like kind, made the Dr. apt to think, that this elephant might not be brought thither by any care or industry of man; but that the surface of this terraqueous globe might in the earliest ages of the world, after the deluge, but before all records of our oldest histories, differ widely from its present geography, as to the distribution of the ocean and dry land, its islands, continents and

shores,

shores, so as to allow this animal and others of its kind, a free and open passage into this country from the continent; for otherwise, how can we ever explain, that that other huge stately animal the mouse-deer, little inferior to the elephant itself, could have been brought to *Ireland* from *North America*, even long before that quarter of the world was known, the only country, where this large animal is found at present.

And can we well imagine, that foxes, otters, badgers, cygers, wolves, with linx's and such ravenous animals, which were discovered by the great snows that fell in 1715 in the island of *Sardinia* and other places, should ever be imported (being useless, noxious beasts of prey) by the industry of man, to propagate in islands, that they might destroy mens food and flocks, and make their lives not only uneasy but unsafe? Nay, how can we suppose, that birds of shortest flight, various sorts of poisonous serpents, and offensive creeping vermin, with all the different tribes of smaller insects, could possibly be found in islands, unless they had been stocked with those inhabitants, when the intercourse between them and the continent was free and open.

But in whatever manner this elephant might first have made its way to *Ireland*; this is beyond dispute, that the bones of elephants have been discovered deep under-ground, in other places as well as in this kingdom, and those remote from the native country of this animal; for, not many years ago, several parts of the skeleton of an elephant, were dug up in a hill near *Erfurt*, a town of the *Upper Saxony* in *Germany*, on which occasion *Wilhelmus Ernestus Tentzelius*, historiographer to the Duke of *Saxony*, published a treatise, entitled, *De Sceletio Elephantino Tonne nuper effosso*: And Dr. *Molyneux* is well persuaded, by the best construction he can make of those imperfect and obscure accounts we have in *Evert Isbrand Idles's curious Travels*, from *Muscovy* to *China* over land, cap. 6. (which he acknowledges he only gathered from the barbarous *Ostiacks*, inhabitants of that country) concerning the vast teeth, bones and limbs of *mammuths*, as he calls them, frequently found and diligently sought after, in the hills and banks of several rivers in *Siberia*, the *Keta*, *Jenize*, *Trugan*, *Montgamssea* and *Lena*, that they are nothing other than the remains and skeletons of elephants buried there, and accidentally discovered by the earth's opening, and falling down on the sudden thaws, after severe long frosts.

But to bring the matter still nearer home, Mr. *Camden* in his *Britannia* is of opinion, that those large monstrous teeth and bones, which he observes were dug up at different times, in several

ral parts of *Great Britain*, must have been the remains of elephants; but then he thinks, they must be of those that *Dion Cassius* the historian tells us, the *Roman* emperor *Claudius* brought over, in his expedition to this island; but that this is really so, the Dr. owns is still but surmise, and has not been so fairly prov'd by him or any other, as that we can rely upon it with satisfaction.

What Mr. *Sommer*, the learned antiquary, has published in his *Chartham News*, is more remarkable, *Phil. Trans.* N^o 272; where he informs us, 'that in 1668, in the village of *Chartham* near *Canterbury* in *England*, digging within 12 roods of a river they found a parcel of strange monstrous bones, some entire, some broken; together with four teeth, perfect and sound, each weighing somewhat above half a pound, and some of them as big as a man's fist; they are all cheek teeth or grinders; the earth wherein they lay being like a sea-earth, or fulfing earth with not a stone in it.'

It is observable, how this account, in several of its circumstances, agrees with that of Mr. *Nevile*, viz. that the teeth were all grinders, four in number, found with other large broken bones near a brook, and in a clayey soil without a stone; but then the weight and magnitude of the largest teeth, found in *Ireland*, so far surpass those in *England*, as not to be a fifth part of them; which shews, that they could not be the teeth of the same animal: It must be owned, the author does not so much as suspect them to be elephant's teeth, but on the contrary is of opinion, that they belonged to another species, the hippopotamus or river-horse, an animal that is still a greater stranger in these parts of the world than the elephant itself; and, therefore, its passage hither can never be accounted for, but by some such like supposition as the Dr. has made above: However, Mr. *Luffkins* in *Phil. Trans.* N^o 274, differs in his judgment from Mr. *Sommer* about these teeth, which he supposes must have been elephant's teeth, as he is positive those large bones, he there describes, and found near *Harwich* in *Essex*, must certainly have been.

Dr. *Molyneux*, not having seen any of these bones or teeth, either those found in *Kent*, or those in *Essex*, pretends not to determine any thing in the matter; only he ventures to say, that if the figures of the teeth given us by Mr. *Sommer* be genuine and well expressed, they seem no way to agree either in shape or make, or in that particular and characteristick work on the grinding superficies, with the teeth of the elephant, or with the
def.

description and figures above given, which are both correct and natural.

Dr. *Molyneux* is apt to think (even from these imperfect hints) that if we had more correct accounts and observations of this kind, made in distant countries, and skilfully registered, with all their instructive circumstances, they might lead us into great and momentous truths, relating to the deluge, to the wise methods of providence, in replenishing all parts of the world with animals soon after the flood, and to the knowledge of several important changes that may have happened on the surface of this our terraqueous globe.

Mr. *Nevile's* account, with Dr. *Molyneux's* curious draught of the teeth, and his learned remarks thereon having been produced and read before the *Royal Society*, they ordered, that what teeth they had of the like sort should be laid before them; to which Sir *Hans Sloane* furnished a still greater variety out of his incomparable collection of *Natural Rarities*; and to obviate all doubts, there being at that time in *Westminster*, the entire skull of a large elephant with the teeth therein, that was likewise ordered to be compared with the figures; upon which it appeared, that the teeth in question could be no other than those of an elephant.

By this enquiry the *Royal Society* was likewise satisfied, that the number of teeth found, being but four, was no objection; it appearing, that the number of *Molares* in this animal is not certain: *Pliny lib. 11. cap. 37.* says expressly, *Dentes Elephantus ad mandendum quatuor, præter eos qui prominent*: And in the remains of that prodigious elephant, described by *Tentzelius*, there were no more than four teeth found; in that at *Westminster* there were six, viz. one in each lower jaw, and two in each of the upper, whereof the inner tooth was about three times as long as the other, and both together longer than those of the lower jaw by about an inch, the upper small teeth being much worn by grinding.

Fig. 5. represents these teeth, shewing the rough grinding surface of the left under tooth, which was considerably concave.

Fig. 6. represents the same roughness on the upper teeth, having a convexity, corresponding with the concavity of the lower, a circumstance not observed by any of those who have described them.

And tho' by the observation of M. *Du Verney*, Dr. *Moulin's* and Mr. *Blair*, who dissected three different elephants, it appears, that each of them had eight *Molares*; yet from them it is also evident,

evident, that in the division of their nature observes no rule; for, Dr. Moulins found the two teeth in each of the upper jaws of that he dissected, to be divided in a different manner; so that the inner tooth on the one side, and the outer on the other, was bigger than its adjoining fellow, yet not so as to be very unequal; and M. Du Verney and Mr. Blair had on both sides the much larger tooth outwards; whereas, the skull at Westminster, on the contrary had only a small tooth outwards, and the much larger grinder within; all which considered, we may safely conclude, that this elephant found in Ireland had but four teeth in his head, when he died; and that the two larger were those of the upper jaws, and the other two, those of the lower.

Again, by the size of the grinding part, we may conclude these to be the teeth of a very young and small elephant, since they are not much above $\frac{1}{2}$ the length of those that are to be seen at Westminster, which belonged to an elephant of no more than between 10 and 11 foot high, not much above $\frac{1}{3}$ of the length of a fossile elephant's grinder in the repository of the Royal Society, which is represented by Fig. 7, all the figures being drawn to the scale of $\frac{1}{3}$ their true dimensions. Hence it is not surprising, that the bones of so young an animal, not having acquired their solidity and firmness, as being in a growing state, as also the roots of the teeth, should be dissolved by lying long in the earth.

Matthew Paris in his history assures us, that in his time Louis IX. (afterwards St. Louis) king of France, made a present of an elephant to his cotemporary Henry III. of England, and that in 1255, after the English had been masters of Ireland for 80 years; of this, says he, *nec credimus quod unquam aliquis Elephas visus est in Anglia præter illum.*

An Account of several Nebulæ or lucid Spots like Clouds, discovered among the fixed Stars by the Telescope. Phil. Trans. N° 347. p. 390.

IN the preceeding Transaction we gave a short account of the several new stars that appeared in the Heavens, within 150 years last past, some of which afford very surprising phenomena; but no less surprising are certain luminous Nebulæ or Spots, which discover themselves only by the telescope, and appear to the naked eye like small fix'd stars; but in reality are nothing other than the light coming from an extraordinary large space in the *Æther*, thro' which a lucid medium is diffused, that shines with its own proper lustre: This seems fully to reconcile that difficulty, which some have moved against *Moses's* description of the

the creation, alledging, that light could not be created without the sun; but in the following instances the contrary is manifest; for, some of these bright spots discover no sign of a star in the middle of them; and the irregular form of those that have, shews them not to proceed from the illumination of a central body; these are, as the aforesaid new stars, six in number, all which we shall describe in the order of time they were discovered, giving their places in the sphere of fixed stars to enable the curious, who are furnished with good telescopes, to view them.

The first and most considerable is that in the middle of *Orion's* sword, marked θ by *Bayer* in his *Uranometria*, as a single star of the third magnitude, and so accounted by *Ptolemy*, *Tycho*, *Brabe* and *Hewelins*; but in reality is two very contiguous stars, environed with a very large transparent bright spot, thro' which they appear with several others; these are accurately described by *Huygens* in his *Systema Saturnium* p. 8. who there calls this brightness *Portentum*, cui certè simile aliud nusquam apud reliquas fixas potuit animadverti, i. e. such a surprising phenomenon, as the like could no where else be found among the fixed stars; affirming, that he observed it by chance in 1636; the middle of it in 1716 was in 12° $00'$ of *Gemini*, with 18° $4'$ S. Lat.

About the year 1661 another of this sort was discovered by *Bullialdus* in *Andromeda's* girdle; this is neither in *Tycho* nor *Bayer*, having been omitted, as are several others, by reason of its smallness; but it is inserted into *Hewelius's* catalogue, who has improperly called it *Nebulosa*, instead of *Nebula*; it has no sign of a star in it, but appears like a pale cloud, and seems to emit a radiant beam to the north east, as that in *Orion* does to the south east; it preceeds in right ascension the northern star in the girdle, or according to *Bayer*, about 1 degree and $\frac{1}{2}$, and in 1716 it had Long. 24° $00'$ of *Aries*, with 33° and $\frac{1}{2}$ N. Lat.

The third is near the ecliptic between the head and bow of *Sagittarius*, not far from the point of the winter solstice: This was found in 1661 by a German Gentleman *M. J. Abraham Ible*, whilst he attended the motion of *Saturn*, then near his *Aphelion*; this is small but very luminous, and emits a ray like the former; its place in 1616 was 4° and $\frac{1}{2}$ of *Capricorn*, with about $\frac{1}{2}$ a degree S. Lat.

A fourth was found by *Dr. Halley* in 1677, when he was making the catalogue of the southern stars; it is in the *Centaur*

and what Ptolomey calls δ $\epsilon\pi\iota$ $\eta\varsigma$ $\tau\epsilon$ $\gamma\alpha\tau\epsilon$ $\epsilon\kappa\upsilon\sigma\tau\alpha\varsigma$, and Dr. Halley, *Nebula in dorso equino*, and it is *Bayer's* ω ; it is in appearance between the fourth and fifth magnitude, and emits but an inconsiderable light, for its breadth, and has no radiant beam; this never rises in *England*, and in 1716 its place was $5^{\circ} \frac{1}{2}$ of *Scorpio*, with 25° and $\frac{1}{2}$ S. Lat.

A fifth was discovered by M. G. Kirch in 1681, preceeding the right foot of *Antinous*; of itself it is but a small obscure spot, but has a star that shines thro' it, which makes it the more luminous; its Long. was in 1716 about 9° of *Capricorn*, with 17° and $\frac{1}{2}$ N. Lat.

The sixth and last was accidentally hit upon by Dr. Halley in the constellation of *Hercules* in 1714; it is nearly in a right line with ζ and n of *Bayer*, somewhat nearer to ζ than n ; and by comparing its situation among the stars its place is sufficiently near, in 26° and $\frac{1}{2}$ of *Scorpio*, with 57° N. Lat. this is but a small spot, but it appears to the naked eye, when the sky is clear and the moon absent.

There are, undoubtedly, more of these which have not yet come to our knowledge, and some perhaps, bigger; but tho' all these spots be in appearance but small, and most of them but of a few minutes in diameter; yet since they are among the fixed stars, that is, since they have no annual parallax, they cannot fail to possess immensely large spaces, and perhaps, not less than our whole solar system; in all those vast spaces it should seem, that there is a perpetual uninterrupted day, which may furnish matter of speculation, as well to the curious naturalist, as to the astronomer.

An Account of a new and safe method of communicating the Small-pox by insition or inoculation; by Dr. Pylarinus. Phil. Trans. N^o 347. p. 393. Translated from the Latin.

THE true inventor of this method is unknown; but it is very certain, that it first prevailed in Greece, particularly in *Thessaly*; from whence getting successively into the neighbouring towns and cities, it at length reached *Constantinople*, where at first, it is true, it lay concealed, as it were, for some years, being rarely made use of, and that only among the meaner sort; but upon the small-pox afterwards raging very much, it began to be more universally known; yet never was used by the better sort, till a certain *Greek* nobleman, of the ancient family of the *Caryophylli*, the Dr's particular friend, asked him about the latter end of the winter 1701, what he thought of the method of

infection, and whether he could adventure to perform it on four of his sons; for, at that time the small-pox raging mortally almost over all the city, made him very apprehensive about his children; the Dr. replied, that he was altogether at a loss what to determine about it, having never before had any experience of this new method of operation: In three days after, the Dr. going again to visit his friend, met at his house with a *Greek* woman, who gave a plain and full account of the whole process, method, place, time and other circumstances of the operation, tho' entirely ignorant of the true cause of the propagation of the small-pox by infection; to these she added experiments, and a great many instances of cases, wherein the operation had always proved safe and successful, as the Dr. afterwards found from the accounts of persons of credit; and therefore, upon weighing the matter thoroughly, he thought it not altogether dissonant to reason and nature, especially from the consideration of the above-mentioned cases; and in some days after, the Dr. being again earnestly importuned by his friend to give his opinion consented to the operation, tho' still with some reluctance; upon which, and receiving proper directions about the regimen to be observed during the whole time of the illness, the nobleman adventured to have the infection performed by the *Greek* woman upon four of his sons; the three younger, between 3 and 7 years old, were slightly indisposed, and a few pustules appearing a week after, they got over both the fever and the danger; but the oldest, above 18 years of age, was very bad; for being taken with a continent and malignant fever, attended with several bad symptoms, together with some exanthemata or pustules, tho' not so very numerous, was scarcely out of danger, the 14 day; and this the Dr. took to be owing to the patient's atrabilious constitution, his depraved juices, and neglecting the proper evacuations at first, in order to prepare his body: The happy issue of this operation induced several other noble families to follow the example; so that now there is none, excepting the more timorous, but is fully persuaded of the good effects thereof; only the *Turks*, who are addicted to the notion of fate, and are less tractable, have hitherto neglected it.

This operation is purely natural, and without any admixture of superstition; it differs widely from sympathetic cures, and much more from that kind of magnetical transplantation whereby distempers (by means of a certain imaginary and imperceptible *Mumia* or virtue) are said to be transmitted from one subject to another: But tho' these magnetic or sympathetic operations have

Some tincture of superstition (as the weapon-salve, the sympathetic powder, &c.) as acting at a distance, and beyond their sphere of activity; yet the insition of the small-pox is purely physical, being produced by mere physical means, obvious both to the eye and to the feeling: The small-pox are propagated or excited by, what is metaphorically called, an insition or transplantation, which is nothing other than an ingrafting of the morbidic ferment or *Pus*, extracted from the pustules of the small-pox, into a sound body, by means of punctures made for that purpose.

The physical manner of the excitation or propagation is thus; the *Pus*, being conveyed into the punctures or incisions, acquires the nature of a true ferment; hence by means of the circulation, being conveyed thro' the proper vessels and canals to the mass of blood, it immediately attacks and infects those particles that are pre-disposed and turgid with a latent contagion, and by communicating a secret poison, it excites, agitates, actuates and puts into motion their fermentative seeds; whence arises an universal ebullition or fermentation, by means of which the more impure and heterogeneous particles, being separated, are critically protruded to the skin, nature in the mean time by such an operation calmly moderating the whole process.

But to come to the insition itself, and the manner thereof; in the first place, a proper season is to be pitched upon for performing the insition, which according to this operation is best done in winter, and she never performed it but in that season; but the Dr. takes the spring to be also a proper season, because of the milder temperature of the air, at that time.

2. She is very curious in her choice of the ferment; not making the insition with *Pus* taken from any subject at random; but when the small-pox are epidemical, she makes a puncture and extracts the matter from the ripe and kindly pustules of some young child, that is otherwise of a sound constitution, and puts it into a very clean glass vessel or some such thing, that is not too cold, and covering it close down she claps it into her servant's bosom to keep it warm, and she immediately repairs to the patient on whom the operation is to be made; she does not use the *Pus* of the small-pox from insition, as not answering the purpose, which yet the Dr. would take to be of a milder nature, and of no less efficacy; but in this, experience is to be consulted.
3. She would have the patient kept in a very temperate room, as to the modifications of the air.
4. In performing the operation itself she makes an oblique puncture with a steel or gold needle on the middle of the forehead close by the hair of the head, as

also

also on the chin and both the cheeks, separating the skin a little from the flesh with the sharp point of the needle; afterwards with the same needle she drops the *Pus* into the wound, tying a bandage over the part; in like manner she also pricks the *Metacarpus* of each hand, and *Metatarsus* of each foot, putting the *Pus* into them, and ties a bandage gently over them, desiring the patient not by any means to scratch or wet those parts: The Dr. would rather chuse to make punctures in the more fleshy parts of the body, as being less obnoxious to inflammations and pain, and not interwoven with tendons: Besides this method of operation, all others are rejected, as absurd, unusual, unsuccessful, and dangerous; in the mean time the patient must use his bed moderately, and not continue in it more than is necessary. 4. She enjoins a good regimen as to the six non-naturals, especially diet; for, she strictly forbids the use not only of wine and meat, but likewise of broth made of the latter, and that for 40 days; and it has very often proved fatal to such patients, as did not follow this direction; for, new pustules have been observed to break out, and other dangerous symptoms to arise.

The incision being thus duly performed, the symptoms do not appear in all patients in the same period of time; for, the ferment acts variously, sooner or latter, according to the various temperament, age and strength of the several patients; tho' the small-pox themselves do almost always begin to appear the seventh day, which is a true critical day; and there have not been instances wanting, tho' it seldom happen, wherein they break out the very first day: The symptoms of this distemper vary according to the diversity of temperaments, the disposition of the juices in the mass of blood, and the natural constitution of each patient, and so accordingly become more remiss or more intense; but they are not unlike the common sort, tho' generally milder; and several patients scarce feel any alteration or pain at all.

The inoculated small-pox are commonly of the distinct kind, and not very numerous; as not exceeding 10, generally 20, or 30 pustules, rarely amounting to 100, and very rarely to 200: It is to be observed, 1. That the inoculation has succeeded, by making one puncture only in the arm, and tho' there were but a few pustules, yet they afterwards escaped the contagion. 2. That it has sometimes happened, that no eruption has succeeded the incision, either because there was no pre-existing variolous disposition in the body, or that the contagion of the ferment was weakened and broken; but afterwards when the distem-

distemper raged epidemically, such patients as had already undergone the operation, were promiscuously seized therewith.

3. The punctures or incisions always turn to pustules; but in some patients they become purulent tumours, without any pustules appearing; in others they degenerate into larger aposthema, that discharge a great deal of Pus; and frequently the same places, especially in the hands and feet, swell with exquisite pain, and upon the discharge of the Pus subside, and afterwards swell up again; in some others, tho' very rarely, abscesses arise after some time, in the glandulous parts and emunctories, which gradually suppurate; nature in this manner varying according to the various constitutions of the patients.

4. And lastly, seldom or never has any bad consequence been observ'd to happen upon this operation, tho' it has been performed on all sexes, ages and temperaments; nay, and when duly applied, and in bodies properly prepar'd by a skillful physician, it proves absolutely safe; for, the small-pox rais'd in this manner are more kindly than the common sort, as being propagated by a ferment or contagion, that has no kind of malignity therein; the ebullition by which the mass of blood is agitated, and the whole work performed proceeds calmly, without any violence, and that under the direction of nature; moreover, the time and properest season of the year for the incision may be pitch'd upon, as also the body that is to undergo the operation may be fitted and prepar'd by proper means for that purpose; and this, indeed, should be reckoned of very great consequence in order to bring the distemper to a safe and happy issue.

An Account of the Occultation of Jupiter by the Moon, observ'd at Wansted, the 14th of July in the Morning 1715. together with an Account of an Eclipse of the Moon observed at the same Place October the 30th following; by Mr. James Pound. Phil. Trans. N° 347. p. 401.

MR. Pound having after midnight carefully corrected the clock by no less than 10 observations of the altitude of *Lucida Arietis*, the error thereof was found $5^{\circ} 13''$ too fast, the extremes not differing above $6''$; and in the morning about 7^h . by as many altitudes of the sun, with a like agreement, the same error was found, $5^{\circ} 14''$ to be deducted from the times shewn by the clock.

July

July 13. p. M. N.		Time by the clock.	Time correct
The third satellite of <i>Jupiter</i> was hid by the moon.		13 27 33	13 22 20
The first satellite was hid		13 32 35	13 27 22
The second satellite was hid		13 34 25	13 29 11
The first contact of the limbs of <i>Jupiter</i> and the Moon		13 34 54	13 29 41
<i>Jupiter</i> wholly hid		13 36 23	13 31 20
The third satellite came out from behind the dark side of the moon		14 7 25	14 2 12
The first satellite		14 12 25	14 7 12
The second satellite		14 14 38	14 9 25
The first limb of <i>Jupiter</i> came out		14 14 45	14 9 32
The following limb of <i>Jupiter</i> , or last cont.		14 16 15	14 11 2
The fourth satellite emerged.		14 18 49	14 13 36

Jupiter and his satellites were to the northward of the visible way of the moon's center: This occultation was observed thro' a telescope, in which the focal length of the object glass was 14 feet and $\frac{1}{4}$, and of the eye-glass two inches and $\frac{1}{4}$; and the aperture of the object glass was 1 inch and one tenth; Mr. Pound could perceive no colours on *Jupiter's* limb, either at his immersion or emersion, when the axis of the telescope was directed to him.

Object.	Apparent Time	An Eclipse of the Moon observed at Wansted, October 30. 1715.	
1	15 09 00	The eclipse had been for some time began	
2	17 00	The moon's diameter measured by a micrometer was	34 04
3	22 25	The chord connecting the horns	30 28
4	35 45	The enlightened part of the diameter continued to the chord between the horns	19 58
5	43 24	The inlightened part of the diameter	13 52
6	49 50	The same repeated	12 02
7	52 43	The same repeated.	11 44

The

8	56	51	The inlightened part of the diameter continued to the chord between the horns.	15	22
9	59	27	The inlightened part of the diameter	10	35
10	16	04	The same repeated	9	43
11	18	34	The same again repeated	9	07
12	23	45	The chord between the horns	32	36
13	26	30	The same repeated	33	07
14	31	16	The same again	33	19
			At which time also the shadow passed thro' the middle of <i>Schikardus</i>		
15	37	13	The chord between the horns, agreeing with the moon's diameter	33	57
16	40	45	The inlightened part of the diameter	11	56
17	43	40	The same produced to the chord between the horns	16	13
18	46	55	The same repeated	17	28
19	47	57	The inlightened part of the diameter	13	38
20	52	57	The same	15	30
21	55	27	The edge of the shadow passed thro' the middle of <i>Gassendus</i>		
22	56	12	The inlightened part produced to the chord between the horns	19	58
23	17	02	The chord between the horns	32	12
24	8	16	The same repeated	30	28
25	10	39		29	56
26	13	00		28	31
27	15	29	The same again	27	33
28	17	37		26	35
29	19	35		25	36
30	21	47	The same again	24	38
31	23	24		23	39
32	24	54		22	40
33	26	27	The same again	21	41
34	27	57		20	42
35	29	08		19	43
36	30	10	The same again	18	44
37	31	07		17	45
38	32	04		16	46
39	32	50	The same again	15	47
40	34	12		13	48
41	17	35	The same again repeated.	11	42

At 17h. 39' the eclipse was supposed to be ended, and was visibly so at 17h. 41'; but by comparing the last observations of the chords between the horns, it follows, that the true end of the eclipse was at 17h 38' 20". At 17h 43' the moon's diameter was 33' 40". The middle of the eclipse cannot be supposed to be very accurately determined by these observations, which were not sufficiently distant from the time of the greatest obscuration; however by comparing several of them together, the middle will be obtained, viz.

By Obs. 3 compar'd with Obs. 24 at	16 15 21
By Obs. 4 compar'd with Obs. 22 at	16 15 58
By Obs. 5 compar'd with Obs. 19 and 20 at	16 16 00
By Obs. 6 and 7 compar'd with 16 at	16 15 48

By reason of the clouds Mr. Pound could not see the beginning of the eclipse, nor make such observations of the moon's immerging into the shadow, as he did of her emerging out of it: By Obs. 11 compar'd with Obs. 15 the digits eclips'd were 8 and $\frac{1}{2}$: The angles were measur'd by a micrometer in a 15 foot telescope; Mr. Pound did not consider how far they were consistent with each other, having set them down here exactly as they were first taken.

This eclipse is the more considerable, as happening very near the moon's *Perigæum*, and, therefore, of use to verify her anomaly, as also to limit the greatest diameter of the shadow of the earth, and consequently, the parallax of the moon: This eclipse may very properly be compar'd with that of the 19. of *October*, 1697, whose middle was at 7h. 41' P. M. at *London*, and its quantity the same as now: The times by the clock were 17' 43" sooner than the apparent time, as was found by the following observations of *Cor Leonis* and *Arcturus*, which were but just discernable thro' the clouds.

Apparent Zenith Distance	Time by the Clock	Apparent Time by Calcular	The Difference	Mean Diff.
of Cor. &c	h. ' "	h. ' "	' "	
76 16 1	13 32 43	13 50 35	17 52	
69 38	36 50	54 44	17 54	
69 09	40 06	57 51	17 45	17 50
58 40	43 09	14 00 59	17 50	
68 08	46 37	04 26	17 59	
of A. &c.				
63 19	17 37 40	17 55 24	17 44	
63 06	39 12	56 48	17 36	
64 41	41 49	59 29	17 40	17 40
63 47	47 40	18 05 17	17 37	

Clock too slow 17 45

The Lat. of *Wanted* is $51^{\circ} 34'$, its Lon. is 8° in time eastwards from the *Observatory* at *Greenwich*.

The account given of this eclipse by *Mr. Derham*, who observed it at *Upminster* is agreeable to this, as far as clouds would permit him to observe.

An Account of the surprising Lights seen in the Air, on the 6th of March, 1715-16, with an Attempt to explain the principal Phenomena thereof; by Dr. Halley. Phil. Trans. N^o 347. p. 406.

ON *Tuesday* the 6th of *March* 1715-16, (the afternoon having been very clear and calm, and somewhat warmer than ordinary) about the time it began to grow dark, that is, much about 7 a clock, not only in *London*, but in all parts of *England*, where the beginning of this surprising phenomenon was seen, there rose in the N. E. parts of the heavens, out of what seemed a dusky cloud, scarce 10 Deg. high, and whose edges were tinged with a reddish yellow, as if the moon had been hid behind it, very long and luminous rays, or streaks, perpendicular to the horizon, some of which seemed nearly to ascend on the *Zenith*; presently after, that reddish cloud was swiftly propagated along the northern horizon into the N. W. and still farther westerly, and immediately sent forth its rays,

after

after the same manner from all parts, now here, now there, without observing any certain rule or order, in their rising; several of these rays, seeming to concur near the *Zenith*, formed there a *Corona*, or image, which drew the attention of all the spectators, who, according to their several conceptions, made very different representations thereof, some likened it to that representation of glory, wherewith our painters surround in churches the holy name of God; others to those radiating stars, with which the breasts of the knights, of the most noble order of the garter are adorned; several others compar'd it to the concave of the great *Cupola* of *St. Paul's Church*, distinguish'd with streaks alternately, bright and obscure, with a space in the middle, less bright than the rest, resembling the lanthorn; whilst others, to express as well the motion as figure thereof, would have it to resemble the flame in an oven, reverberated and rowling against its arch'd roof; and there were some that took it to have a greater resemblance to that tremulous light, which is cast against a cieling by the beams of the sun, reflected from the surface of water in a bason that is shaken a little, whose reciprocal vibrating motion is very much imitated: But all agree, that this *Spectrum* lasted only a few minutes, and shew'd itself variously tinged with colours, as yellow, red and a dusky green; nor did it keep in the same place; for, when first it began to appear, it was seen a little to the northward of the *zenith*; but gradually declining towards the south, the long *stria* of light, which arose from all parts of the northern semi-circle of the horizon, seemed to meet together, not much above the head of *Castor* and the northern *Twin*, and there it soon disappear'd.

After the first *Impetus* of this ascending vapour was over, the *Corona*, above described, appear'd no more; but still without any order as to time, place or size, luminous rays like the former, continued to arise perpendicularly, now oftener, and again seldomer, now here, now there, now longer, now shorter; nor did they proceed as at first out of a cloud, but oftener would emerge at once out of the pure sky, which was at that time more than ordinary serene and still; nor were they all of the same form; most of them seemed to terminate in a point upwards, like erect cones; others, like truncated cones or cylinders, so much resembled the long tails of comets, that at first sight they might well be taken for such; again, some of these rays would continue visible for several minutes; when others, and those the much greater part, just shew'd themselves and died away; some seemed to have but little motion, and to stand fix'd, as it were, among

the stars, whilst others with a very perceptible translation mov'd from east to west under the pole, contrary to the motion of the heavens, by which means they would sometimes seem to run together, and at other times to fly from each other, affording thereby a very surprising sight to the spectators.

After this sight had continued about an hour and $\frac{1}{2}$, those beams began to rise much fewer in number, and not near so high; and that diffus'd light, which had illuminated the northern parts of the hemisphere, seem'd gradually to subside, and settling on the horizon, form'd the resemblance of a very bright *Crepusculum*: That this was the state of this phenomenon at first, is abundantly confirm'd by the unanimous consent and concurring testimony of several credible persons; for, by the accounts receiv'd from almost all the extreme parts of the kingdom, there was found very little difference in the description, from what appear'd at *London* and *Oxford*; unless that in the north of *England* and in *Scotland*, the light seem'd somewhat stronger and brighter.

Hitherto the Dr. is oblig'd to relate the observations of others, wherein he is afraid several very material circumstances may be omitted, he himself having had no notice of this matter, till between nine and ten o'clock, being then at a friend's house; but upon the first information of the thing, he immediately ran to the windows, which happened to regard the south and south-west quarter; and soon perceiv'd, that tho' the sky was very clear, yet it was tinged with any odd kind of light; so that the smaller stars were scarce to be seen, and much the same as when the moon of four days old appears after the twilight; and whilst the Dr. and his friend regarded the heavens with attention, they perceiv'd a very thin vapour to pass before them, which arose precisely from the east part of the horizon, ascending obliquely, so as to leave the zenith about 15 or 20 degrees to the northward; but the swiftness with which it proceeded was very considerable, seeming not inferior to that of lightning, and exhibiting, as it pass'd along, a sort of momentaneous *nubecula*, which discover'd itself by a very dilut'd and faint whiteness, and no sooner form'd, but it was gone before the eye could well take it in, leaving no signs behind it; nor was this a single appearance, but for several minutes that they regarded it, about six or seven times in a minute, the same was again and again repeated; these undulations of vapour, as it were, regularly succeeded each other, and as was conjectur'd, at intervals very nearly equal, all of them in their ascent producing a like transient *nubecula*: By this particular

cular they were first assured, that the vapour they saw, whatever it were, became conspicuous by its own proper light, without the help of the sun beams; for, these *Nubeculae* did not discover themselves in any other part of their passage, but only between the south-east and south, where being opposite to the sun they were deepest immersed in the cone of the earth's shadow, nor were they visible either before or after; whereas, the contrary must have happened, had they borrowed their light from the sun.

They then made all the haste possible to a place where there was a free prospect of the northern horizon; being come there, not much past 10 o'clock, they found on the western side, *viz.* between the west and north-west, the representation of a very bright twilight, contiguous to the horizon, out of which there arose very long beams of light, not exactly erect towards the *Vertex*, but somewhat declining to the south, which ascending by a quick and undulating motion to a considerable height, vanished in a little time, whilst others, tho' at uncertain intervals, supplied their place; but at the same time, thro' all the rest of the northern horizon, *viz.* from the north-west to the true east, there did not appear any sign of light to arise from, or join to the horizon; but on the contrary, what appeared to be an exceeding black and dismal cloud seemed to hang over all that part of it; yet it was no cloud, but only the serene sky more than ordinary pure and limpid, so that the bright stars shone clearly in it, and particularly the *tail of the Swan* then very low in the north, the great blackness manifestly proceeding from the neighbourhood of the light, which was collected above it; for, the light had now put on a form quite different from all the Dr. has hitherto described, and had fashioned itself into the shape of two *Laminae* or streaks, lying in a position parallel to the horizon, whose edges were but ill terminated; they extended themselves from the north by east to the north-east, and were each about a degree broad, the undermost about 8 or 9 degrees high, and the other about 4 or 5 degrees over it; these kept their places for a long time, and made the sky so bright, that the Dr. thinks a man might easily have read ordinary print by the help thereof.

Whilst they stood astonished at this surprising sight, and were expecting what was farther to come, the northern extremity of the upper *Lamina* gradually bent downwards, and at length closed with the extremity of the other that was under it, so as to shut up on the north-side an intermediate space, which still

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continued open to the east; not long after this, in the said included space, they saw a great number of small columns or whitish streaks appear suddenly perpendicular to the horizon, and reaching from the one *Lamina* to the other, which instantly disappearing were too quick for the eye, so that they could not determine, whether they arose from the under or fell from the upper *Lamina*; but by their sudden alterations they made such an appearance, as might well be taken to resemble the conflicts of men in battle.

And much about the same time, there began on a sudden to appear, low under the pole and very nearly due north, three or four lucid area's like clouds, discovering themselves in the pure, but very black sky by their yellowish light; these, as they broke out at once, so after they had continued a few minutes, disappeared as quick as if a curtain had been drawn over them; nor were they of any determined figure, but both in shape and size might properly be compared to small clouds illuminated by the full moon, but brighter.

Not long after this, there arose from above the aforesaid two *Lamine* a very large pyramidal figure, resembling a spear, sharp at the top, whose sides were inclined to each other in an angle of about four or five degrees, and which seemed to reach up to the zenith or beyond it; this was carried with an equable, and no very slow motion from the N. E. where it arose to the N. W. where it disappeared, still keeping in a perpendicular situation, or very near it; and passing successively over all the stars of the *Little Bear*, it did not efface the smaller ones in the tail, which are but of the fifth magnitude, such was the extreme rarity and transparency of the matter of which it consisted.

This single beam was so far remarkable above all those, that for a great while before either preceded, or followed it, that if its situation amongst the circumpolar stars had at the same instant been accurately observed, for instance, at *London* and *Oxford*, whose difference of longitude is well known, we might thereby be enabled to pronounce with some certainty, by the diversity of its aspect, concerning the distance and height thereof, which undoubtedly were very considerable, tho' hitherto we can no ways determine them; but as this phenomenon found all those that are skilled in the observation of the heavens unprepared, and unacquainted with what was to be expected; so it left all of them surprised and astonished at the novelty thereof; When therefore, for the future any such thing shall happen, all those that are curious in astronomical matters are hereby admonished

nished and intreated to set their clocks to the apparent time, for instance, at *London*, by allowing so many minutes, as is the difference of meridians; and then to note at the end of every half hour precisely, the exact situation of what at that time appears remarkable in the sky; and particularly, the azimuths of those very tall pyramids so eminent above the rest, and therefore, likely to be seen farthest; to the intent that by comparing those observations, taken in the same moment in distant places, the difference of their azimuths may serve to determine how far those pyramids are from us.

It being now past 11 o'clock, and nothing new offering itself to the view, but the repeated *phases* of the same phenomenon, the Dr. and his friend continued no longer in the chill of the night air; but returning to his own house, he made haste to his upper windows, which conveniently enough regarded the N. E. parts of the heavens; and he soon found, that the two *Laminae* or streaks abovementioned, parallel to the horizon, had entirely disappeared; and the whole phenomenon reduced itself to the resemblance of a very bright *Crepusculum*, settling on the northern horizon, so as to be brightest and highest under the pole itself; from whence it spread both ways into the N. E. and N. W. Under this in the middle thereof there appeared a very black space, the segment, as it were, of a lesser circle of the sphere, cut off by the horizon; it seemed to the eye like a dark cloud, but was not really so; for, by the telescope the small stars appeared thro' it more clearly than usual, considering how lost they were, and upon this as a basis rested the *Lumen auriforme*, which was no other than a segment of a ring, or zone of the sphere, intercepted between two parallel lesser circles, cut off in like manner by the horizon; or the segment of a very broad *Iris*, but of one uniform colour, viz. a flame-colour inclining to yellow, the center thereof being about 40 degrees below the horizon; and above this there were seen some rudiments of a much larger segment, with an interval of dark sky between, but this was so exceeding faint and undetermined, that he could make no proper estimate thereof.

The Dr. attended this phenomenon till near three in the morning, and the rising of the moon; but for above two hours together, it had no manner of change in its appearance, nor diminution or increase of light; only sometimes, for very short intervals, as if new fuel had been cast on a fire, the light seemed to undulate and sparkle, not unlike the rising of vaporous smoke out of a great blaze, when agitated; but by this attendance and watch-

watching the Dr. assured himself of one thing, *vis.* that this *Iris* like figure did by no means owe its origin to the sun beams; for, about three in the morning, the sun being in the middle between the N. and E. this *aurora* did not follow him, but ended in that very point, where he then was; whereas, in the true north, which the sun had long passed, the light remained unchanged and in its full lustre.

Hitherto the Dr. has endeavoured to represent by words what he saw, but being sensible how insufficient such a verbal description of a thing so extraordinary and uncommon may be to most readers, he has annexed a figure, exhibiting that particular appearance of the two *Laminae*, which he saw at London between the hours of 10 and 11; more especially, because he does not find, among the several accounts he had seen, any one that had taken notice of it: A B Fig. 8. Plate VI. represents the under *Lamina*, somewhat broader and brighter than the upper *Lamina* C D; it had near its under edge the *Lucida Lyrae*, and below its northern extremity on the left hand *Cauda Cygni*; and as well above as below these, as in the intermediate space between them, and indeed all round about that part of the heavens, the sky was so unusually dark and black, as if all that light, that had shewn itself before, had been then collected into those two streaks; only at Q between the west and north-west and nowhere else, there arose, out of a brightness adjoining to the horizon, conical beams as M, L, N, after the same manner as at first.

In the mean time the streak C D at its northern extremity bent downwards, and joined with the under streak A B at E, and included the space D C E A B, which still kept open at the other extremity towards the east; and in the mean time, out of the very clear sky, some luminous spots, situated and figured as at G, G, G, G, presented themselves to the eye, in colour much like the *Laminae*; these did not appear all together, but came successively, yet so as that two or three of them were seen at a time; and as their coming so their disappearing was instantaneous; likewise at the same time the several little white columns, represented by F, F, F, F, possessed that part of the space between the two streaks next to E, and by their sudden and very irregular motion, and the vanishing of some, whilst others at the same time emerged, gave occasion to the notion of those who fancied battles fought in the air; lastly, from about the middle of C D, there suddenly arose a cone or obelisk of a pale whitish light, as H, larger than any hitherto seen; which, moving from
east

east to west, with a motion sufficiently regular, was carried to K in the north-west, and there disappeared: In order to shew by the same figure the appearance of the last hours after midnight, it is to be observed, that the light at Q is represented much larger than what appeared in the west about 10 o'clock, so as to exhibit that other truly; in this case the point Q must by the imagination be supposed transferred to the intersection of the horizon and meridian under the pole; the scheme indeed could by no means be contrived to answer the surprising variety this phenomenon afforded; since even the eye of no one single observer was sufficient to follow it in the suddenness and frequency of its alterations.

Thus far the Dr. has attempted to describe what was seen, not being able to say any thing more of the first and most surprising part thereof, which however frightful and amazing it might seem to the vulgar beholder, would have been to him a most agreeable and wished for sight: The like is not recorded in the *English* annals since the year 1574, in the reign of queen *Elizabeth*; then, as we are told by *Camden* and *Stow*, eye-witnesses of sufficient credit, much the same surprising phenomena were seen, with almost all the same circumstances as the present, for two nights successively, viz. on the 14. and 15. of *November* of that year.

Nor, indeed, was this so rare a sight, as it has been since; for, we find in a book, entitled a description of meteors, reprinted at *London* in 1654, whose author writes himself *W. F. D. D.* that the same thing, which he there calls burning spears, was seen at *London* on *January* 30. 1560. and again, by the testimony of *Stow*, on the 7. of *October* 1564. And from foreign authors we learn that in 1575 the same was twice repeated in *Brabant*, viz. on the 13. of *February* and 28. of *September*; and seen and described by *Cornelius Gemma*, professor of medicine in the university of *Louvain*; who, in a tract on the prodigies of those times, after several ill-boding prognostics, very properly describes the *Cupola* and *Corona*, that he saw in the *Chasma*, as he calls it, of *February*, in the following manner; 'a little after, says he, spear and new flames arising, the sky seemed to be all on a flame, from the north quite up to the zenith; and at last, the face of the sky was, for a whole hour together, changed into the uncommon form of a dice-box, the blue and white changing alternately, with no less swiftness and vertiginous motion than the sun beams do, when reflected from a mirror.' Here it is pretty remarkable, that all these four

appearances, already mentioned, fell exactly upon the same age of the moon, *viz.* about two days after the change.

As to the other of *September* in the same year 1575, *Gemma* writes thus; 'the form of the *Chasma*, of the 28. of *September* following, immediately after sun-set, was indeed, less dreadful, but still more confused and various; for, in it, were seen a great many bright arches, out of which gradually issued spears, cities with towers and men in battle array; after that, there were excursions of rays every way, waves of clouds and battles; they mutually pursued and fled, wheeling round in a surprising manner.' From hence it is manifest, that this phenomenon appeared in our neighbourhood three several times, and that with considerable intervals, within the compass of one year; tho' our *English* historians have not recorded the two latter; nor did *Gemma* see that of *November* 1574, as it is most likely, by reason of clouds: After this in 1580 we have the authority of *Michael Maestlin* in *lib. de Cometa* 1580. (himself a good astronomer, and still more famous for having been the great *Kepler's* tutor in the sciences) that at *Baknang* in the country of *Wirtemberg* in *Germany*, these *Chasmata*, as he likewise calls them, were seen by himself no less than seven times within the space of a twelve month; the first of these, and the most considerable, happened on the very same day of the month with the present, *viz.* on *Sunday* the 6. of *March*, and was attended with much the same circumstances; and again the same things were seen in a very extraordinary manner on the 9. of *April* and 10. of *September* following; but in a less degree, on the 6. of *April*, 21. of *September*, 26. of *December* and 16. of *February* 1581; the last of which, and that of the 21. of *September* must needs have been more considerable than they then appeared, because the moon, being near the full, necessarily effaced all the fainter lights: However, of all these no one is mentioned in our annals to have been seen in *England*, nor in any other place that the *Dr.* could find.

The next was that of 1621. on the 2. of *September* O. S. seen all over *France*, and well described by *Gassendus* in his physics, who calls it *Aurora borealis*; this, tho' little inferior to what the *Dr.* saw, and appearing to the northward both of *Rouen* and *Paris*, is no where said to have been observed in *England*, over which the light seemed to lie: Another was seen all over *Germany* in 1623, thus described by *Kepler*; 'on 7. of *November* 1623 a glowing ball was seen generally all over *Germany*; in *Austria* it was also said to give a peal like thunder; which yet

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'I suppose to be false, the descriptions, that are extant, concerning no such thing.' And since that time for upwards of 80 years, we have no accounts of any such sight either at home or from abroad; the first we find, was one of small continuance, seen in *Ireland* by Mr. Neve on the 16. of *November* 1707, *Vide Phil. Transf.* N^o 320. And in the *Miscellanea Berolinensia*, published in 1710, we learn, that in the same year 1707, both on the 24. of *January* and 18. of *February*, O. S. something of this kind was seen by M. Olaus Romer at *Copenhagen*; and again on the 23. of *February*, the same astronomer observed there such another appearance, but much more considerable, of which notwithstanding he only saw the beginning, by reason of interposing clouds: But the same was seen that night by M. Gottfried Kirch at *Berlin*, upwards of 200 miles from *Copenhagen*, and lasted there till past 10 o'clock at night: To these add another small one of short duration, seen near *London*, a little before midnight between the 9. and 10. of *August* 1708, by the bishop of *Hereford*, and communicated to the *Royal Society*; so that, it seems, in little more than 18 months this sort of light has been seen in the sky, no less than five times in 1707, and 1708.

Hence we may reasonably conclude, that the air or earth, or both are sometimes, tho' but seldom and at great intervals, disposed to produce this phenomenon; for, tho' it be probable, that many times, when it happens, it may not be observed, as falling out in the day time, or in cloudy weather, or bright moon-shine; yet that it should, be so very often seen at sometimes and so seldom at others, is what cannot well be accounted for that way: The material cause of these appearances may probably be the vapour of water exceedingly rarified by subterraneous fire, and tinged with sulphureous steams; which vapour is now generally taken by our naturalists to be the cause of earthquakes, and as earthquakes happen with great uncertainty, and have been sometimes frequent in places, where for several years both before and after they have not been felt; so in like manner these appearances, we are now accounting for, may be supposed produced by the eruption of the pent vapour thro' the pores of the earth, when it is not in a sufficient quantity, nor sudden enough to shake its surface, or to open itself a passage by rending it; and as these vapours are suddenly produced by the fall of water upon the nitro-sulphureous fires under ground, they might well be supposed to acquire from thence a tincture, which might dispose them to shine in the night, and a tendency contrary to that of gravity; as we find the vapours of gun-powder, when

heated *in Vacuo*, shine in the dark, and ascend to the top of the receiver, tho' exhausted.

Nor should the Dr. seek for any other cause than this, if in some of these instances, and particularly in this whereof he treats, the appearance had not been seen over a much greater part of the earth's surface than can be thus accounted for; it having been visible in this last from the west side of *Ireland* to the confines of *Russia* and *Poland* on the east, extending over at least 30 degrees of longitude and in latitude from about 50 degrees over almost all the north of *Europe*; and in all places exhibiting at the same time the same surprising circumstances, as appeared by the *public News*: Now this is a space much too wide to be shaken at any one time by the greatest earthquakes, or to be affected by the perspiration of that vapour, which being confined and wanting vent, might have occasioned an earthquake; nor can we this way account for that remarkable particular, attending these lights, of their being always seen on the northside of the horizon, and never to the south.

Wherefore, laying aside all hopes of being able to explain these things by the ordinary vapours or exhalations of the earth or waters, we are forced to have recourse to other sorts of effluvia of a much more subtile nature, and which perhaps, may seem more adapted to bring about those surprisingly quick motions we have seen; such are the magnetic effluvia, whose atoms freely permeate the pores of the most solid bodies, meeting with no obstacle from the interposition of glass or marble or even gold itself; some of these do, by a perpetual efflux, arise from the parts near the poles of the magnet, whilst others of the like kind of atoms, but with a contrary tendency, enter in at the same parts of the stone, thro' which they freely pass; and by a kind of circulation surround it on all sides, as with an atmosphere, to the distance of some diameters of the body: This thing *Des Cartes* has endeavoured to explain in *Princip. Philos. lib. 4.* by the hypothesis of the circulation of certain skrew-like or striate particles, adapted to the pores they are to enter.

But without enquiring how sufficient the *Cartesian* hypothesis may be for answering the several phenomena of the magnet, and that the fact may be the better comprehended, the Dr. exhibits the manner of the circulation of the atoms, concerned therein, as they are exposed to view, by placing the poles of a *Terrella* or spherical magnet on a plane, as the globe on the horizon of a right sphere; then strewing fine steel dust or filings very thin on the plane all round it; the particles of steel, upon a continued gentle

gentle knocking on the underfide of the plane, will gradually conform themselves to the figures in which the circulation is performed; thus let A B C D Fig. 9 be a *Terrella*, and its poles A the south, and B the north; and by doing as prescrib'd, it will be found, that the filings will lie in a right line, perpendicular to the surface of the ball, when in the line of the magnetic axis continued; but for about 45 degrees on either side, from B to G or I, and from A to H or K they will form themselves into curves, more and more incurvated as they are remoter from the poles, and withal more and more oblique to the surface of the stone, as the figure truly represents, and as may readily be shewn by the *Terrella* and *Apparatus* for that purpose in the *Repository* of the *Royal Society*: Hence it may appear, how this exceeding subtile matter revolves, and particularly how it permeates the magnet with more force and in greater quantity in the circumpolar parts, entering into it on the one side, and emerging from it on the other, under the same oblique angles; whilst in the middle zone about C and D, near the magnet's equator, very few if any of these particles do impinge, and those very obliquely.

Now, by several, and those very evident arguments, it appears, that our globe of earth is no other than one large magnet, or rather two magnets, the one including the other, as the shell includes the kernel (for, so and not otherwise we may explain the changes of the variation of the magnetic needle) but to our present purpose the result is the same: It suffices, that we may suppose the same sort of circulation of such an exceeding fine matter to be perpetually performed in the earth, as we observe in the *Terrella*; which subtile matter freely pervading the pores of the earth, and entering into its southern pole, may pass out again into the æther at the same distance from the northern pole, and with the like force, its direction being still more and more oblique, as the distance from the poles is greater; further the Dr. supposes, that this subtile matter, no otherways discovering itself but by its effects on the magnetic needle, entirely imperceptible, and at other times invisible, may now and then by the concurrence of several causes, very rarely coincident, and to us hitherto unknown, be capable of producing a small degree of light, perhaps from the greater density of the matter, or the greater velocity of its motion; after the same manner, as we see the effluvia of electrical bodies by a strong and quick friction emit light in the dark, to which sort of light this seems to have a great affinity.

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This being allow'd, the Dr. thinks we may readily assign a cause for several strange appearances, and those the most difficult to be otherwise accounted for; as why these lights are rarely seen any where else but in the north, and never, that we hear of, near the equator; as also why they are more frequently seen in *Iceland* and *Groenland* than in *Norway*, tho' nearer the pole; for, the magnetic poles, in this age, are to the westward of our meridian, and more so of that of *Norway*, and not far from *Groenland*, as appears by the variation of the needle, observ'd in 1716, full 12 degrees at *London* to the west.

The erect position of the luminous beams or *Striae*, so often repeated that night, was occasion'd by the rising of the vapour or lucid matter, nearly perpendicular to the earth's surface; for, any line, erected perpendicularly on the surface of the globe, will appear erect to the horizon of an eye, placed any where in the same spherical superficies, as *Euclid* demonstrates in a plane, that any line, at right angles to it, will appear perpendicular to that plane from any point thereof; that it should be so in the sphere is a curious proposition, not very obvious, but demonstrated from *Prop. 5. lib. 1. Theodosii Spher.* for, by it all lines erect on the surface pass thro' the center, where meeting with those from the eye, they form the planes of vertical circles thereto; and by the converse hereof it is evident, that this luminous matter arose nearly perpendicular to the earth's surface, because it appeared in this erect position; and whereas in this appearance (and perhaps in all others of the kind) those beams which arose near the east and west, as L, M, N, were farthest from the perpendicular, inclining on both sides towards the south, whilst those in the north were directly upright; the cause thereof may well be explained by the obliquity of the magnetic curves, forming still obtuser angles with the meridians of the *Terrella*, as they are farther from its poles.

Hence also it is manifest, how that surprising *Corona*, that was seen to the southward of the *Vertex*, in the beginning of the night, and so very remarkable for its tremulous and vibrating light, was produced; to wit, by the concurrence of several of those beams arising very high out of the circumjacent regions, and meeting near the zenith, the effluvia, of which they consisted, mixing and interfering with each other, and thereby producing a much stronger, but uncertain wavering light: And since all accounts agree, that this *Corona* was tinged with various

rious colours; it is more than probable, that these vapours were carried up to such a height, as to emerge out of the shadow of the earth, and to be illuminated by the direct beams of the sun; whence it might come to pass, that this first *Corona* was seen colour'd, and much brighter than what appeared afterwards in some places, where the sight thereof was repeated more than once, after the sun was gone down much lower under the horizon: Hence it will also be understood, that this *Corona* was not one and the same in all places, but different in every different horizon; exactly after the same manner, as the rainbow, seen in the same cloud, is not the same bow, but different to every different eye.

Nor is it to be doubted, but the pyramidal figure of these ascending beams is optical; since, according to all likelyhood, they have parallel sides, or rather are tapering the other way; but by the rules of perspective, their sides should converge to a point, as we see in paintings the parallel borders of strait walks, and all other lines, parallel to the axis of vision, meet as in a center; wherefore, these rays which arose highest above the earth, and were nearest to the eye, seemed to terminate in cusps sufficiently acute, and for that reason have been supposed by the vulgar to represent *spears*; others again seen from afar, and perhaps not rising so high, as the former, would terminate, as if cut off with planes parallel to the horizon, like truncated cones or cylinders; and these have been taken to look like the *battlements* and *towers* on the walls of cities, fortified after the ancient manner; whilst others still farther off, by reason of their great distance, a great part of them being intercepted by the interposing convexity of the earth, would only shew their pointed tops, and because of their shortness, have been called *swords*.

Next the motion of these beams furnishes us with a new, and, as it seems to the Dr. a most evident argument to prove the diurnal rotation of the earth (tho' that be a matter, which at present is generally taken by the learned to be past dispute) for, those beams, which rose up to a point, and did not presently disappear, but continued for some time, had most of them a sensible motion from east to west, contrary to that of the heavens; the largest and tallest of them, as being the nearest, being swiftest, and the more remote and shorter beams, being slower; by which means the one overtaking the other, they would sometimes seem to meet and jostle; and at other times to separate and fly from each other; but this motion was only optical, and occasioned by the eye of the spectator being carried away with the earth into
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the east; whilst the exceeding rare vapour of which those beams consisted, being, as the Dr. takes it, raised far above the atmosphere, was either entirely left behind, or else followed with but part of its velocity, and therefore could not but seem to recede and move the contrary way; and after the same manner as the stars, that are near the zenith, pass over those vertical circles, which border on the meridian, much swifter than those stars, which are more distant therefrom; so these luminous rays would seem to recede faster from east to west, as their bases were nearer the eye of the spectator, and *à contrà*, slower as they were farther off.

Nor are we to think it strange, if after so great a quantity of luminous vapour had been carried up into the *Æther* out of the pores of the earth, the cause of its effervescence at length abating, or perhaps the matter thereof being consumed, these effluvia should at length subside, and form those two bright *Lamine* described above, and whose edges, being turned to us, could emit so much light, that one could read by them: The Dr. chuses to call them *Lamine*, because, undoubtedly, tho' they were but thin, they spread horizontally over a large tract of the earth's surface; and whilst this luminous matter dropped down from the upper to the under plate, the several little white columns, only visible for the moment of their fall, were formed between them by its descent; these, by the swiftness with which they vanished, and by reason of their great numbers, shewing themselves and disappearing without any order, exhibited a very odd appearance; those on the right seeming sometimes to drive and push those on the left, and *vice versa*.

These are the principal phenomena, of whose causes the Dr. would with more certainty have given his thoughts, had he seen the whole from the beginning to end, and have added his own remarks to the relations of others, especially, if he could by any means have come at the distances thereof: If it shall be thought an odd supposition to assume the effluvia of the magnetic matter for this purpose, which in certain cases may themselves become luminous, or rather sometimes carry with them out of the bowels of the earth a sort of atoms, proper to produce light in the *Æther*; the answer is, that we are not hitherto acquainted with any other kinds of effluvia of terrestrial matter, which may serve the purpose; than the magnetic atoms, and those of water highly rarified into vapour; nor do we find any thing like it in the celestial bodies, unless we except the effluvia projected from the bodies of comets to a vast height, and which seem by a *vis centrifuga*

centrifuga to fly with an incredible swiftness from the centers both of the sun and comet, and to go off into tails of a scarce conceivable length: What may be the constitution of these cometical vapours, we the inhabitants of the earth can know but little of, only that they are evidently excited by the heat of the sun; whereas this meteor, if it may be so called, is seldom seen but in the polar regions of the world, and that most commonly in the winter months; but whatever may be the cause thereof, if this be not, the Dr. has followed the old axiom of the schools; viz. *entia non esse temerè nec absque necessitate multiplicanda*.

Lastly, on this occasion, the Dr. mentions what, near 25 years before, he had published in *Phil. Transf.* N^o 195; viz. that supposing the earth to be concave, with a lesser globe included within it, in order to make that inner globe capable of being inhabited, there might, not improbably, be contained some luminous medium between the globes, so as to make a perpetual day below: That very large tracts of the *Ætherial* space are possessed by such a shining medium is evident from the instances given in the first paper of this *Transaction*; and if such a medium should be thus inclosed with the earth, what should hinder but we may be allowed to suppose, that some parts of this lucid substance may, on very extraordinary occasions, transude thro' and penetrate the *Cortex* or shell of our earth, and being got loose, may afford the matter of which this meteor consists: This seems to be favoured by one considerable circumstance; viz. that the earth, because of its diurnal rotation, being necessarily of the figure of a flat spheroid, the thickness of the *Cortex*, in the polar parts of the globe, is considerably less than towards the equator, and therefore, more likely to give passage to these vapours; whence a reason may be assigned, why these lights are always seen in the north: But the Dr. would lay no more stress upon this conceit than it will bear.

It having been observed, that, in 1575, and 1580, wherein this appearance was frequent, it was seen not far from the times of the two equinoxes, the Dr. thinks it may be worth while for the curious to bestow some attention on the heavens in *September* and *October*, 1716; and, in case it should again happen, to endeavour to observe, by the method the Dr. has here laid down, what may determine, with some degree of exactness, the distance and height thereof, without which we can scarcely come to any just conclusion.

An Account of the Phænomenon of March 6th, 1716, as it was seen on the Ocean, near the coast of Spain; together with an Account of the return of the same sort of appearance, on March 31, and April 1 and 2, 1717. Phil. Trans. N° 348. p. 430.

IN the preceeding *Transaſtion*, there is given as good an account of the ſurpriſing meteor, ſeen in the heavens, on the 6th of *March*, 1715-16, as could be gather'd from the ſeveral accounts of very diſtant ſpectators; and ſince that time all that could be added thereto, is, that, at *Paris*, the light was ſo inconfiderable, as not to be regarded; but by a letter dated *April* 19th, 1716, from on board a ſhip in *Nevis* road in *America*, there is the following account; 'On the 6th of *March* at 9 o'clock in the evening, we being then in the Lar. of 45° 36' (off the N. W. coaſt of *Spain*) a clear cloud appear'd eaſt of us, not far diſtant from our zenith, which afterwards darted itſelf forth into a number of rays of light every way, like the tail of a comet, and of ſuch a great length, to reach within a ſhort way of the horizon; there likewiſe appear'd a body of light N. N. E. of us, which continued almoſt as clear as day, till after 12 o'clock; it appeared at a good diſtance from us, and it darkened on a ſudden.' Hence it would ſeem, that the vapour, which cauſ'd this appearance, aroſe indifferently out of the ocean, and the land; by which we may conclude its great ſubtility, ſince it could permeate ſo great a quantity of water, and ſtill retain its velocity; which is a circumſtance deſerving the farther conſideration of the curious.

But ſince this, moſt of the ſame phænomena have been repeated three ſeveral nights ſucceſſively; viz. on the laſt of *March*, and the firſt and ſecond of *April*: The beſt and full'eſt account of the two firſt is given by Dr. *Taylor* in a letter, dated *April* 2, from *Cotterſtock*, near *Oundle* in *Northamptonſhire*, who thus deſcribes them; 'On *Saturday* night laſt, and laſt night, I ſaw appearances of the ſame kind, with thoſe of *March* 6, but not to be compared with them for extent and ſtrength; they both began ſoon after ſun ſet, and continued till after 12, but how much longer I cannot tell; they were both about 10 or 15 degrees to the weſtward of the north, and took up about 80 degrees of the horizon; and the *Aurora* roſe about 30 degrees high, with a dark bottom, like what was ſeen in the firſt; from whence ſprung out ſeveral bodies
of

of light, which immediately ran into streams, ascending about 30, or at least 40 degrees high; there was no flashing or waving light, but, in all other respects, these lights were of the same kind with what we saw at *London*; indeed in that last night, there was one phenomenon like the flashing light; for, a body of light about 15 or 20 degrees long, and parallel to the horizon, rose till it came about six degrees above the black basis, and then sent up two strong streams of light about 40 degrees high, which at top dash'd against each other, and disappear'd.

At *London* the first night, *March 31*, it did not begin to radiate, till towards midnight, and was seen but by few curious persons; the beams not rising very high, and scarce appearing over the houses, were taken little notice of; but, by the relation of these that saw it, it was much more considerable than the next night after *Easter-day*; for it then emitted but few, and those very short beams, mostly terminating in a sharp point, and presently disappearing; only that beginning to stream as soon as it became dusky, it was very observable that those rays which arose out of the west end of the luminous arch, next the sun, were enlightened by its beams, and shew'd themselves much brighter than those, which sprung up under the pole, or to the eastward thereof; and after 9 o'clock, till midnight, there arose no more beams; and the luminous arch with its black basis settled down very low in the northern horizon.

These same two nights, by the observation of Mr. *Lingen*, the like appearance was seen at *Dublin*, about the hours of 9 or 10; at which time, in the former night, it was near as bright as in a moon-light night: And from *France* we had an account, that on both those nights, the same was seen at *Paris*, with much the same circumstances as at *Dublin*; so that it seems, this meteor, tho' no ways comparable to that of the 6th of *March*, was seen at no less than 150 leagues distance, and probably much farther.

The following night, *April 2*, when it began to be dark, a luminous arch appear'd in the north, with a very narrow black bottom under it, very low and depress'd to the horizon; nor was it seen at or about *London* to project any pointed rays as the former: But what was most remarkable that evening was, what was seen at *London* by Mr. *Faulks* about 9 that night, he being in the open air at that time, saw in an instant a bright ray of very white light appear in the east, out of the pure sky

then very serene and calm; it very much resembled the tail of a comet, and was about 20 degrees inclined from the perpendicular to the right, beginning about γ of *Bayer* in the *Corona borea*, and terminating about the *Informis*, call'd by some *Cor Caroli*; this, after appearing but for a very short time, disappear'd at once, as in a moment, when on a sudden such another beam was instantly produced, not exactly in the same place, but in the same situation; its lower end, being about 20 degrees high, was terminated exactly between α and γ in the right hand and arm of *Hercules*, and the middle of it pass'd over σ and ρ in the girdle of *Bootes*, and thence proceeded westwards, leaving *Cor Caroli* four or five degrees to the northward; after it had continued in this position near 10 minutes immoveable among the stars, it began to move slowly towards the north; and the lower end passing over the northern edge of the *Crown*, and the ray itself over *Cor Caroli*, it grew fainter and vanish'd, continuing in all about 20 minutes; this latter, with some interruptions, extended between *Castor* and *Pollux* very far into the west; and about that time the same, or such another beam was seen at *St. Asaph* by *Dr. Stanley*.

Some of Sir Isaac Newton's Experiments of Light and Colours, repeated before the Royal Society; by Mr. Defaguliers. Phil. Trans. N° 348. p. 433.

THE manner of separating the primitive colours of light to such a degree, that if any one of the separated lights be taken apart, its colour shall be found unchangeable, was not publish'd before *Sir Isaac Newton's Optics* came abroad: for want of knowing how this was to be done, some gentlemen, of the *English College of Liege*, and *M. Mariotte* in *France*, and some others, took those for primitive colours, which are made by immitting a beam of the sun's light into a dark room thro' a small round hole, and refracting the beam by a triangular prism of glass placed at the hole; and by trying the experiment in this manner, they found, that the colours, thus made, were capable of change, and thereupon gave out, that the experiment did not succeed: And the *Editor* of the *Acta Erudit.* for *October 1713*, p. 447, desir'd, that *Sir Isaac Newton* would remove this difficulty; 'The objections,' says the editor, made by learned men both in *France* and *England* against the theory of colours, were happily answer'd by the sagacious *Mr. Newton*, as is abundantly evident from the *Philosophical Transactions*, N° 84, 85, 88, 96, 97.

121, 123, 128; whence many do wish, he would please to give his sentiments on the difficulty started against that theory by the late ingenious M. Mariotte (who was both an indefatigable and successful inquirer into nature) in his treatise of colours, p. 207 & seq. At the distance of about 25 or 30 feet, he receiv'd on a paper, a ray admitted thro' a small hole into a dark room, and transmitted thro' a triangular prism; then he receiv'd with another prism in a very oblique position the violet colour, possessing a space upwards of three lines, and trajected thro' a slit of two lines; upon which, he observ'd that some part of it was changed into a red and yellow colour; and in like manner he found, that a part of the red light was changed into a blue and violet colour; but upon admitting this transmutation, it is manifest, from the *Acta Erudit.* for 1706 p. 60 & seq. that Sir Isaac Newton's theory falls to the ground: M. Mariotte took the distance of 30 feet, lest any should object, that by taking a less distance, the heterogeneous rays had not been perfectly separated; but to me M. Mariotte's experiment would seem decisive, had the entire blue light been changed into another. Thus far the *Editor* of the *Acta Erudit.*

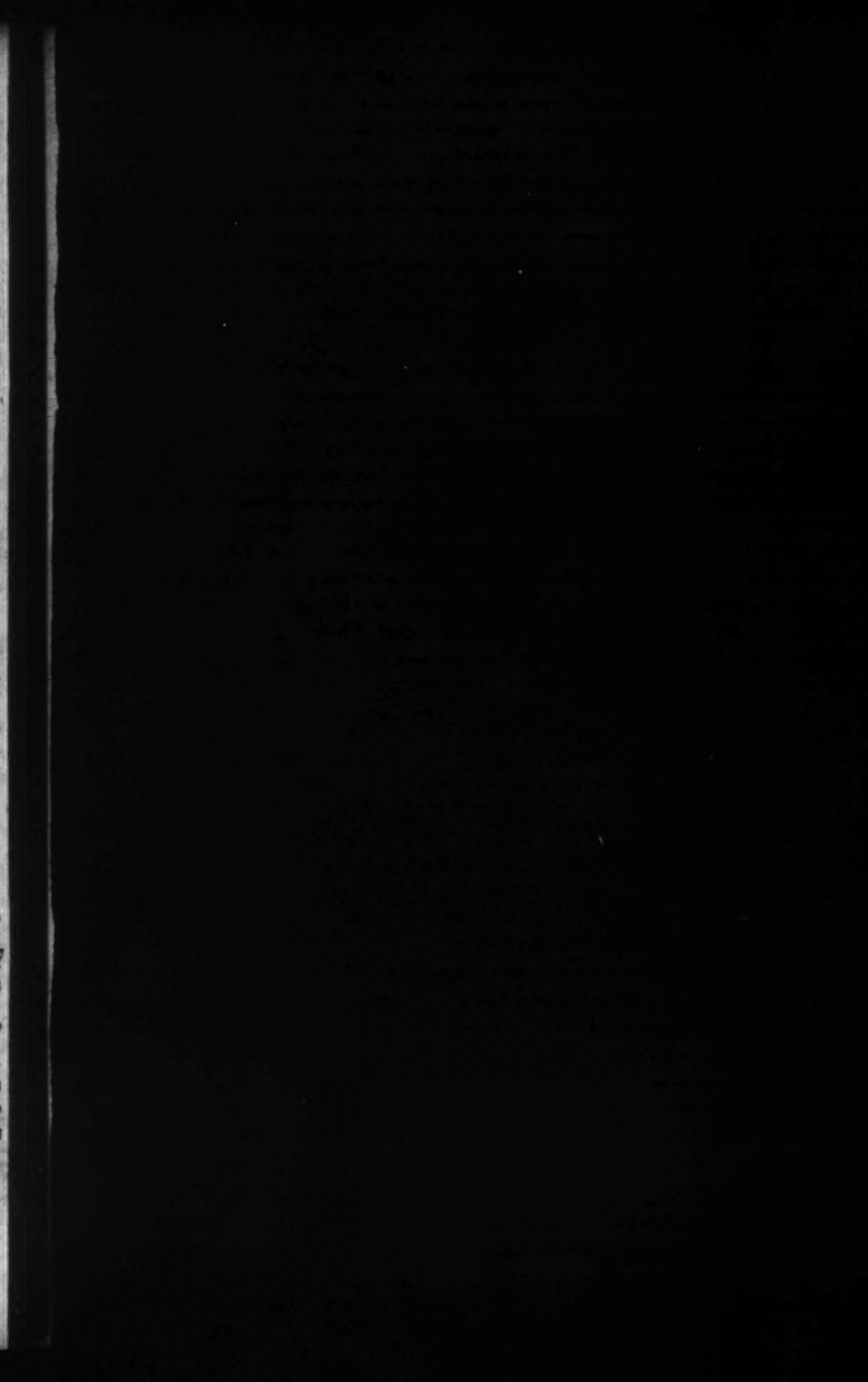
In answer to which, it is to be observ'd, that the red and the yellow, which came out of the violet, and the blue and the violet, which came out of the red, might proceed from the very bright light of the sky, next encompassing the sun; and that several sorts of rays, which come from several parts of the sun's body, are intermix'd in all parts of the colour'd *Spectrum*, which falls upon a paper at any distance from the prism; in this manner of trial, for making the experiment succeed, the light of the bright clouds, immediately surrounding the sun, should be intercepted by an opaque skreen, placed in the open air without, at the distance of 10 or 20 feet from the hole, thro' which the sun shines into the dark room; and in the skreen there should be a small hole, for the sun to shine thro'; the hole may be either round or oblong, and not above $\frac{3}{4}$ or $\frac{1}{2}$ of an inch broad; so that the skreen may intercept not only the bright light of the clouds, next encompassing the sun's body, but also the greatest part of the sun's light; for thereby the colours will become less mixed; the beam of light which passes thro' this hole must afterwards pass thro' the other hole into the dark hole, and the prism must be placed parallel to the oblong hole in the skreen, and the re-
fracting

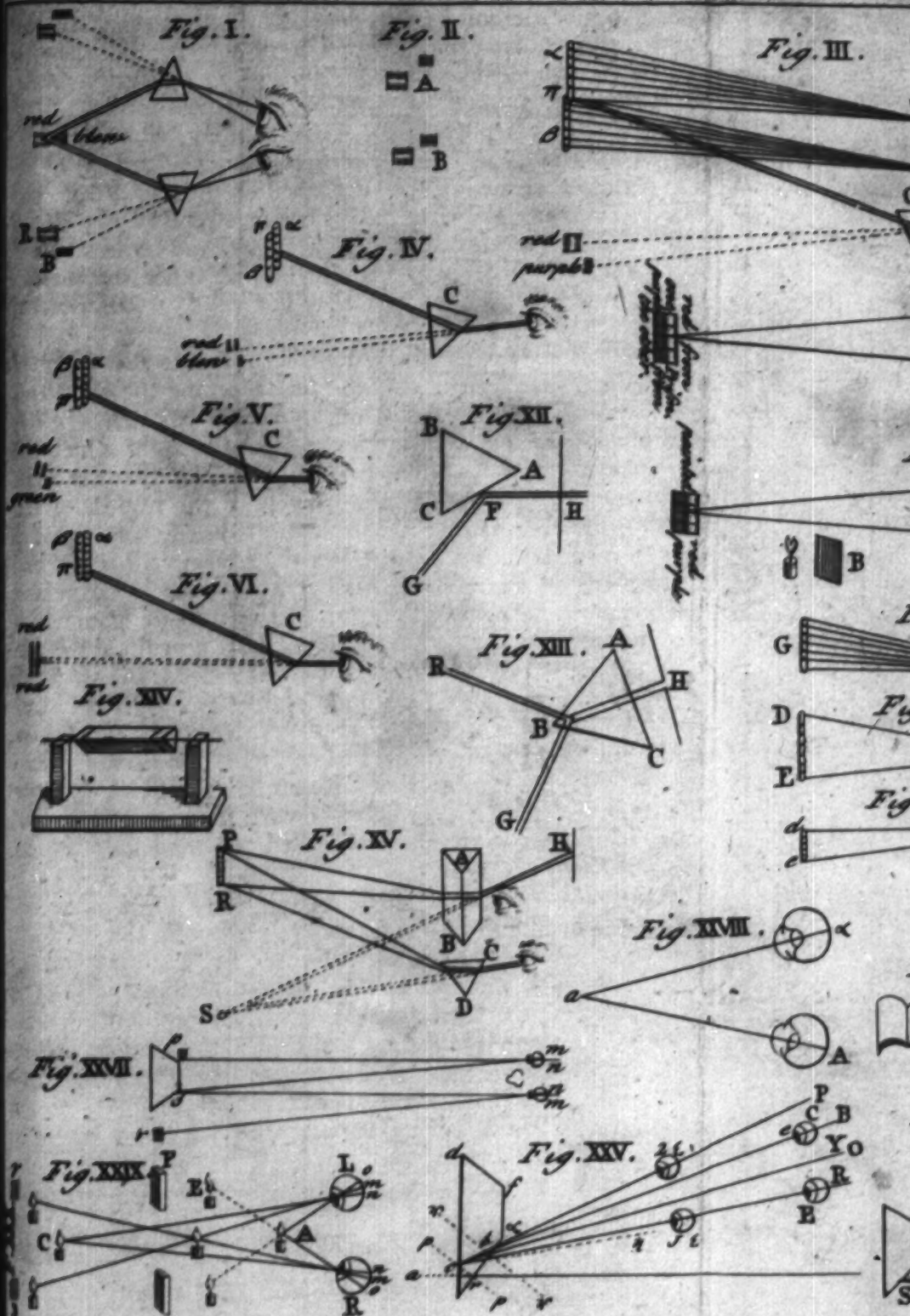
refracting angle thereof be 60 degrees, or upwards: In this manner the experiment may be tried with success; but the trial will be less troublesome, if made in such a manner, as is described in *Prop. 4. lib. 1. of Sir Isaac Newton's optics.*

Sir Isaac Newton, therefore, upon reading what has been cited out of the *Acta Erudit.* desired Mr. Desaguliers to try the experiment in the manner described in the said *Prop.* and he tried it accordingly with success before several of the *Royal Society*, and afterwards before M. Monmort and others of the *Royal Academy of Sciences*: How this and other concomitant experiments were tried, and how they succeeded, is described as follows.

Experiment I. Having sewed together end-wise two pieces of ribbon, four inches long each, the one blue and the other red, whose common breadth was $\frac{3}{4}$ of an inch; Mr. Desaguliers caused it to be held in such a manner, that the light which fell from the clouds thro' the window was so reflected, that the angle made by the rays of light, which came in at the middle of the window, with the plane of the ribbon produced, was equal to the angle made by a line drawn from the ribbon to his eye and the said plane of the ribbon; his eye was placed as far behind the ribbon as the window was before it, the distance from which to him was about 12 feet; then looking thro' a prism at the ribbon, it appeared broken asunder, where the blue and red half were joined; if the prism was held with the refracting angle downwards (or laid with one of its planes flat upon the nose) the blue half of the ribbon appeared to be carried down lower than the red as at B, R, Fig. 1. Plate VII. but if the refracting angle of the prism was turned upwards (as when the prism has one of its planes laid flat to the forehead) then the blue half of the ribbon was lifted up, as at β , p .

The prism was of white glass, having every angle of 60 degrees; but when, instead of it, one of a greenish sort of glass, such as object glasses of telescopes are made of, was used, having the refracting angle thro' which Mr. Desaguliers looked thro' of about 48 degrees, the same phenomenon was more distinct, this glass having no veins, but the red and blue were nearer to a strait line; in such a manner that if A represent the ribbon seen thro' the first prism, B will represent the ribbon seen thro' the second prism Fig. 2. if the refracting angle of the last prism had been as great as that of the first, the light being transmitted thro' too great a body of greenish glass, the phenomenon would not have succeeded so well.





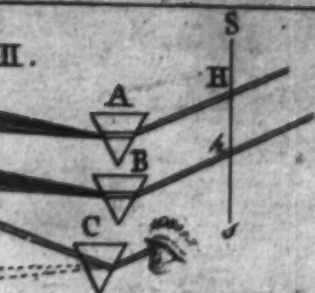


Fig. VI.



Fig. VII.

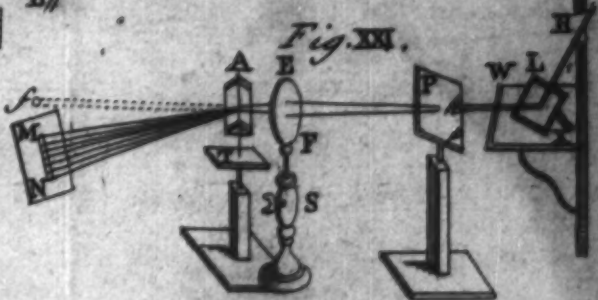
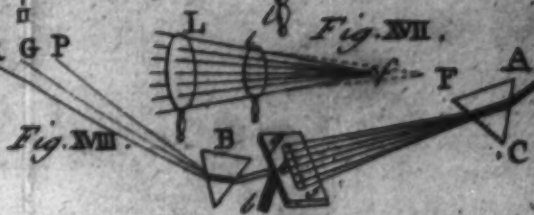


Fig. XVI.



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The blue ribbon being somewhat too pale, and the red a little dull; he repeated the experiment with a skeen of blue, and one of red worsted, joined together in the middle, as the ribbons were before; and the colours of both being very intense, the experiment succeeded better with both prisms: All that were present, trying the experiment, found it to succeed, and that every circumstance answered to the account given in *Prop. 1. Theor. 1. Book 1. of Sir Isaac Newton's optics*, as far as the directions there given were followed: So that it appeared, that the blue being carried lower down than the red in the first case, and lifted higher up in the second, was owing to the greater refraction of the blue ray; for, tho' each part of the ribbon or worsted reflected all manner of rays, yet the phenomenon was very apparent; as also that the blue ribbon or worsted reflected the blue rays more copiously than the red rays; and the red ribbon or worsted the red rays more than the blue ones; because the red of the blue half, seen thro' the prism, was less intense than that of the red half, and the blue or purple of the red half, seen thro' the prism was less intense than that of the blue half.

It is to be observed, that if the ribbon or worsted be laid upon any enlightened body, the phenomenon will not succeed so well; the colours of the body, seen thro' the prism, mixing with those of the ribbon or worsted; even a black body will not answer, if light falls upon it; but there must be a black cloth behind, in such a manner that no light falling upon it can be reflected, so as to disturb the phenomenon; and if a short-sighted person look thro' the prism, a concave *Lens* between his eye and the prism will render the phenomenon more distinct than it would otherwise be.

Exper. II. Some days after, the sun shining, Mr. Desaguliers made two holes *H, h* Fig. 3. in the window shutter *S, s* of a darkened room, thro' which letting the sun beams pass, by means of two prisms *A, B* (one near each hole) he opened the rays coming from the sun into the two coloured *Spectra a, b*; where the following colours were very distinct, *viz.* red, orange, yellow, green, blue, purple and violet; now the reason of their being more distinct than ordinary was, that the prisms he made use of were of the greenish glass above-mentioned; which is very free from those veins, by which the colours are too much thrown into each other, by the best white prisms of the common sort.

The above-mentioned coloured *Spectra* being thrown into the room, to the distance of about 20 feet from the window, where the

the sun's light came in, he caused a piece of white paper π , $\frac{1}{4}$ of an inch broad and five inches long, to be held within the refracted rays (at the distance of 10 feet from the windows) which produced these colours in such a manner, that by turning the prisms round their axes, he could make the red ray of the *Spectrum*, made by the one prism, fall upon one half of the paper, and the purple ray of the *Spectrum*, made by the other prism, fall upon the other half; for, the *Spectra* were both vertical, the lines which terminated the long sides of them towards each other just touching, as appears in the figure. Then at the distance of nine foot, looking thro' the prism C at the paper thus coloured, the red half appeared very much separated from the purple, the one seeming lifted up from the other; the red or the purple appearing the highest, according as the refracting angle of the prism was either held upwards or downwards; the phenomenon is much more distinct this way than any other; for, the paper not only seems divided into two, when it is coloured by a red and a purple ray, but also by a red and a blue, as in Fig. 4. by a red and a green ray, as in Fig. 5. or indeed by any two colours that are different, how near soever their places in the *Spectra* be to each other: The halves of the paper appear, when viewed thro' the prism, to be farther from each other, when the paper is tinged with such colours, as are farther from each other in the series of colours in the *Spectrum*; and nearest, tho' still divided, when neighbouring colours fall upon the paper, as yellow and green, or a light and a deep green; but the paper appears no way divided, when coloured with the red of the two *Spectra*, as in Fig. 6. if those red colours are equally intense; and so of the other colours.

Exper. III. Mr. Desaguliers held a *Lens* of about three foot radius at the distance of six feet from the oblong paper (on which a red and purple ray falling, made it look half red and half purple) and he projected the image of the said coloured paper at the distance of about six feet on the other side of the *Lens*, on a white sheet of paper; where it was observable, that when the red half was distinctly painted on the white paper (which was known by the edges of the image being regularly terminated) then the blue half of the image was confused; but if the white paper was brought about two inches nearer to the *Lens*, the image of the blue half became distinct, and that of the red half confused.

He tried the experiment with a paper, coloured half red and half blue (the red with carmine and the blue with smalt) making

ing the candle to enlighten the paper (the room being otherways dark) and the experiment succeeded in the same manner, the experiment made in this manner is the same Sir *Isaac Newton* gives an account of *Book 1. Part. 1. Theor. 1.* of his optics; only it is to be observed, that when the oblong paper is coloured with red and blue from the prisms, the focal place, where the red part of the image is distinct, is more distant from the place, where the blue part of the image is distinct than when the paper is coloured with painter's powders, and much more vivid.

Fig. 7. shews the projection of the paper, tinged with the rays; and Fig. 8. its projection when painted; where a black thread is wrapped round the red and the blue part, that the distinctness of the image of the thread may shew, when the red or when the blue part of the image of the paper is most distinct.

N. B. When the candle enlightens the painted paper, set an opaque body, as *B*, between the candle and *Lens*, lest the image of the candle, being also projected, should disturb the experiment.

Exper. IV. Upon making a hole of $\frac{1}{2}$ of an inch in diameter in the window-shutter of the darkened room, he suffered a sun-beam to come into the room, which he intercepted with a prism at the distance of five inches from the hole; and after its refraction in passing thro' the prism, he received it on a sheet of white paper, where it was coloured, making an oblong image or *Spectrum* of the sun about nine inches long and two inches in breadth; which breadth was nearly equal to the diameter of the round image of the sun, received on a paper at the same distance from the hole, which here was 18 feet; or if the sun be too high, a looking-glass, put in the room of the prism, will throw a white round *Spectrum* upon the paper, which held at the said distance of 18 feet, will have its diameter equal to the breadth of the coloured *Spectrum*: The colours of the *Spectrum*, as in Fig. 9. were red, orange, yellow, green, blue, purple and violet; tho' the violet was so faint in this as to be scarce perceivable.

N. B. The axis of the prism in this, and all the other experiments hereafter mentioned, must be perpendicular to the ray that falls thereon; and the plane, into which the ray enters, must be held in such a position, that the angle, which such a ray makes with that plane when it enters, may be equal to the angle made by the middle line of those rays, which emerge after refraction on the other side of the refracting-angle of the prism, with the plane out of which they emerge, that is, the angle *BDG* = the angle *A E H*.

If the plane AC , on which the sun-beam falls, be turned nearer to a perpendicular to the sun-beam than before, the *Spectrum* will be much longer; if more inclined to the said beam, the *Spectrum* will be shorter, and in both cases less distinct; see the *Spectrum* DE and the *Spectrum* de in Fig. 10, 11. where Hh represents the hole in the window-shutter in each case; AC , ac , the plane of the prism on which the rays enter; BC , bc that out of which they emerge; P , p the perpendicular, and C , c the refracting angle: If the plane AC be still more oblique to HF , all the light will be reflected, and there will be no coloured image or *Spectrum* made by refraction at all, as in Fig. 12. But if it be held so as to be more nearly perpendicular to the sun-beam than in Fig. 10, the whole beam will, it is true, enter the prism; but meeting with BC the lower surface of the prism, or rather the surface of the air contiguous to it, some of the light will by the plane BC be reflected to da , passing almost perpendicularly thro' AB , and the rest will emerge thro' BC , and by refraction form the imperfect *Spectrum* DE , as in Fig. 10. If the sun-beam enter AC perpendicularly and in the middle thereof, the light will be all reflected, as in Fig. 13. some of it by the plane BC to R , and the rest by the plane AB to q ; but if the beam fall nearer to A (still perpendicularly) it will all be reflected by the plane AB ; if nearer to B , it will be all reflected by the plane BC .

In order therefore to have the coloured *Spectrum*, as it ought to be, care must be taken, that the emerging coloured light may form the same angle with the plane BC , as the immerging light does with the plane AC ; that is, the angle AEH must be equal to BDG , as was said above in Fig. 9. which may also be seen on the enlightened dust in the air; but the best way is to turn the prism on its axis, and at the same time look at the coloured *Spectrum*, which will rise and fall, and become longer or shorter, as you turn the prism; and between the ascent and descent of the image, it will appear stationary; there stop the prism, and the reflection will be such, as is required for all the experiments hereafter mentioned.

In order to have the prism move freely on its axis, and stop any where, Mr. Desaguliers fixed each end of it into a triangular collar of tin, from the end of which there came a wire, which was the axis of the prism produced; and thus he laid it on two wooden pillars, with a notch on the top to receive the wires, and he fixed it to a small board just broad enough to stand firm, as in Fig. 14.

Expe-

Exper. 5. He took the prism CD and thro' it he looked at the coloured *Spectrum* RP, which then appeared round and white, as at S, just as if it had been the sun's light received on a paper from the hole H, and seen with the naked eye; in this case the prism CD must be held in a direct line with AB, and the refracting angles in the two prisms must be equal; this *Spectrum*, appearing white but just in one point, is not so readily found; but the best way is to look thro' the same prism AB, which forms the *Spectrum*; and this may be easily done, if it be pretty long, and then RP will be seen white and round, and as at S, as if coming directly from H, as in Fig. 15.

Exper. V. Mr. Desaguliers held a broad *Lens* L, ground to a radius of two feet and a half, in such a manner, that the whole coloured *Spectrum* fell thereon; and after refraction all the colours appeared to converge, if received on a paper at pp; but when the paper was held in the *Focus* at F in the position π F π , the *Spectrum* was round and perfectly white by the uniting of all the coloured rays; if the paper was held at Π Π , the colours appeared to diverge from each other; but then the red was uppermost, which before used to be lowest, and so on, in an inverted order: He tried the same experiment with a *Lens* of one foot radius, with one of nine inches, and with another of seven, and the success was the same: Fig. 16. represents the colours at R, O, Y, G, B, P, V.

N. B. Care must be taken that the very extremity of the red and the end of the violet be taken in by the *Lens*; otherwise the *Spectrum* will not be perfectly white at the *Focus* of the glass: There is no fixed distance of the prism from the *Lens*, but it ought to be brought so near the prism, that the two ends of the *Spectrum* may fall nearer the axis of the *Lens* than the edges of the *Lens*; because there the refraction is not so regular: Behind the *Lens* L, which made the colours converge into white at the distinct base or *Focus* F, Mr. Desaguliers placed the *Lens* l, which made the white be at f the distinct base of the two glasses combined, and the experiment succeeded as before, as in Fig. 17.

When the paper was held in the *Focus* of the *Lens*, so as to receive the white image of the coloured *Spectrum*, projected by the *Lens*; if he intercepted the red ray with a card, the white appeared tinged with purple; and if he intercepted the violet or purple ray, or both, the white appeared tinged with red; and if the red was intercepted at the same time, the *Spectrum* appeared to be a mixture of yellow, green and blue; if any single colour was suffered to fall upon the *Lens*, the rest being intercepted,

that colour would continue the same, only it would be more intense in the *Focus* of the *Lens*.

Exper. VII. He took a board, as in Fig. 18. qbs , which stood reclining on a prop e , having a hole of a quarter of an inch in diameter at b , and behind it a prism B, supported on two props, as abovementioned, so as to turn easily about its axis; and having set this board on the ground with the prism behind it at B, by turning the prism AC about its axis, he first made the red ray of the coloured *Spectrum* pass thro' the hole b , and fall obliquely upon the second prism B; this ray, after its refraction in passing thro' the second prism, was carried up to the ceiling of the room at the place, marked R; then he made the purple ray fall upon the board, and pass thro' the hole b , as the red had done before; and after refraction thro' the prism B, it was carried up to the ceiling at P; and the green ray, being afterwards made to pass the second prism in the same manner, went up to G; and so of all the intermediate rays, which were by this second refraction thrown to the intermediate places on the ceiling between R and P.

Care is to be taken, that the second prism be placed oblique to the rays, which come thro' the hole b , lest they be reflected, as they would be, if the board being in the position QS, and the second prism in the position LNM, the ray from the first prism be pb ; for, then it will be reflected upwards to e instead of being refracted, as in Fig. 19. Neither must the plane of immersion be too oblique, lest the incident ray be reflected downwards thereby, as the ray R b is by the prism B thrown to E in Fig. 20. Several have failed in this experiment, for want of setting the second prism in a due inclination.

Tho' the colours by the second refraction appeared unchanged on the ceiling, when seen by the naked eye, yet if viewed thro' a prism, they afforded new colours, excepting some part of the red, and some part of the violet, which was owing to their not being fully separated; for which reason, he made the following experiment to prove, that if the colours be well separated, they are truly homogeneous and unchangeable: *N. B.* when the prisms are good, and no clouds near the sun, the extremity of the red or violet will afford unmixed colours in this experiment, otherwise not.

Exper. VIII. Upon making a hole in the window-shutter two inches wide, as in Fig. 21. he applied a tin-plate thereto, which sliding up and down covered all this hole in the wood, and only transmitted a small beam thro' its own hole H, whose diameter

was

was $\frac{1}{16}$ of an inch; this beam he reflected horizontally to the other end of the room, by means of the looking glass *L*, placed on the board of the window *XW*; but to correct the irregularity of the reflection of the looking glass, he made use of the frame of paste-board, *Pp*, which had a hole therein *b*, likewise of $\frac{1}{16}$ of an inch, and placing it at *Pp* he suffered some of the reflected beams to pass thro' it, so as to fall upon the *Lens EF* (convex on both sides, and ground to a radius of four feet and a half) at the distance of nine feet, so that the image of the hole *b* was projected to *f* on the other side of the glass, at the distance of nine feet more: Just behind the *Lens*, which by a screw in the stand *S* might be raised or let down, so as always to receive the beam along its axis, he placed a prism *A* (upright on one extremity, and easily moveable about its axis, by reason of its wire turning freely in a hole in the solid piece of wood *T*, which stood on another stand behind the *Lens*) as near as he could to the *Lens EF*, so that the image *b*, instead of being round, white and projected to *f*, was cast sidewise on a white paper, stretched on a frame, and appeared coloured, and 30 or 40 times its breadth, as at *MN*; the colours in this case were very vivid and well separated, only the violet had some pale light, darting from its extremity, upon account of some yeins in the prism *A*; and the light not coming directly from the sun, but reflected; which ought not to have been, had the sun been low enough to have thrown the rays a good way into the room, without the help of a looking-glass: To shew, that the colours in this *Spectrum* were simple and homogeneal rays, he made the following experiment.

Exper. 9. Having made a hole *b* in the paper, which received the coloured *Spectrum*, he suffered the red light to pass; which, being refracted by a second prism, fell upon another paper at *T*, where it appeared still red, whether seen with the naked eye, or with prisms of different refracting angles: To the eye that saw it thro' the prism *V*, it appeared, it is true, lower as at *r*, but red, round and unchanged: He made the experiment upon all the colours, which by this means appeared to be simple and homogeneal, as in Fig. 22. where the same letters denote the *Lens*, prism and first paper: Thro' the same *Lens* and prism the same *Spectrum* was made to fall upon a book; then thro' the prism *F* it appeared unchanged; and the letters in the book, which crossed the *Spectrum*, were as distinct, as when seen with the naked eye, as in Fig. 23.

N. B. The axis of the prism *F* should be perpendicular to the long axis of the *Spectrum sm*, thrown on the book, which will appear as at *σ μ*, and the prism be in the position, represented at *F* with its flat side towards the nose; for that is the most convenient position for looking at the *Spectrum* in these experiments.

He suffer'd the purple ray only to pass thro' the hole *h*, and fall upon a book at *P*; the characters of which appear'd at *π*, and were as distinct thro' the prism *Q*, as when seen with the naked eye; and he had the same success with all the other rays, as in Fig. 24. But if a sun-beam as *r* come thro' the hole *H* directly upon the book at *W*, an eye looking at it thro' a prism at *X* will see this beam at *Y* oblong and colour'd, and the characters upon which it falls, confus'd, as in Fig. 24. It is to be observ'd that the lens should be very good, without veins or blebs, and ground to no less a radius than has been mentioned in the experiment; tho' a radius of a foot or two longer is not amiss; the prism should be of the same glass as the optic glasses of telescopes; the white glass, of which prisms are usually made, being commonly full of veins; and the room in these last experiments ought to be very dark.

A few days after, having procur'd very good prisms, made for the purpose, of the above mentioned glass, he repeated all the experiments before several members of the *Royal Society* with better success; and had the *Spectrum* very regularly terminated, without any pale light darting from its extremities; for a further account of experiments to this purpose, vide Sir Isaac Newton's *Optics Book 1. Part 1.*

A plain and easy Experiment to confirm Sir Isaac Newton's Doctrine of the different refrangibility of the Rays of Light; by the same. Phil. Trans. N° 348. p. 448.

AFTER the *Experimentum Crucis* made by two prisms, Mr. Desaguliers would not have given the following, but that it is so easy to be made, that thereby such as want the *Apparatus* (or are unwilling to be at the pains to make the *Experimentum Crucis*) may at any time satisfy themselves of the truth of the above-mentioned doctrine.

Let the candle *A*, as in Plate VII Fig. 23. be set before the bar of a chimney looking-glass, as *H H*, which is a piece of looking-glass plate, consisting of four planes, seen in the section thereof *a f d β*, viz. *d β*, which is quicksilver'd behind; *f x* a plane parallel to it; *f d* one of the side-planes bezell'd towards

towards $d\beta$, or inclined to it in an angle of about 40 degrees (tho' from 30 to 40 will do, but the greater the angle the better, if it exceed not 45 degrees) $a\beta$ the other side-plane, inclined in the same angle to βd : The rays of the candle, which come from A to γ , fall obliquely on the plane $a\beta$, so that instead of going on to a , they are by refraction made to incline more towards the perpendicular pp , namely to go on in the line γc , and then are reflected from the point c on the quick-silver'd surface, in the direction cx , so as to make the angle $xcd = \gamma c\beta$; now as the rays, which would go on to x , if not refracted, emerge obliquely from the plane $a\beta$, they leave the direction cx , and decline from the perpendicular pp , and, being differently refracted, open into four differently colour'd rays, viz. bR a red ray, bYO a ray made up of orange and yellow; bGB a ray made up of green and blue or a sea-green, and bP a purple ray: If from the place E you look full upon the point b , the *Spectrum* or image of the candle at b will appear double, but not mix'd; that is, there will appear a sea-green spot and a red spot, as it were, one upon another; but not so as to produce a mix'd or intermediate colour; then if the right eye, or eye at E be shut, there will only appear a green spot to the eye at e ; if the eye at e be shut, the eye at E will see a red spot only: If you come nearer to b , so that the eyes at $s1$, $s2$ receive the most and the least refrangible rays, there will be a double *Spectrum*, viz. a red and a purple one just touching, or upon each other, and the phenomenon will answer as before: If keeping both eyes open, you direct their axes towards O a point nearer than the usual place of the compound *Spectrum* S , as in Fig. 26. which point is in a line from the nose N to the point S ; or in other words, if you look full at O , or at the end of your finger held in O , the red and the blue, or purple spot, will appear to be divided from each other after the manner, represented at pr , as in Fig. 27. where the red will appear to be on the right hand, and the blue on the left.

In order to make plain what is meant by *seeing* the *Spectra* p and r , whilst we look full at O , Mr. Desaguliers explains the distinction between *looking* and *seeing*, the better to shew, how this phenomenon proves, that the sensation of different colours is caus'd by rays differently refracted.

Definition 1. The optic axis is a line, which, going thro' the center of the connexity of all the coats and humours of the eye, falls upon the middle of the *Retina*, as aa or Aa Fig. 28.

Des.

Def. 2. To look at any point, is to turn both eyes towards it in such a manner, that the optic axes, making an angle at the said point as *a*, the rays from *a* may have the optic axis for their axis, and (by their converging upon the *Retina* after refraction in the eye) may paint the image of the said point, upon the middle of the *Retina* of each eye, where the optic axis in each eye falls.

Def. 3. To see without looking, is to direct the optic axes to some other place than to the point, which is then seen; and in such a case, the image of the point seen will be projected upon a part of the *Retina* of each eye, where the optic axis does not fall: namely, either nearer to the nose *N*, as in Fig. 26, at the points of the *Retina* mark'd *nn*, or farther from the nose than the middle of the *Retina*, as at *oo* in Fig. 29.

Whatever is seen, by being look'd at with both eyes, always appears single, by reason of the communication between the middle of the *Retina* in one eye, and the middle of the *Retina* in the other; there being no such communication between any other part of the *Retina* in one eye, and the correspondent part of the *Retina* in the other, when these correspondent parts are equally distant from the nose: There is, it is true, a communication between the nervous fibres on the right side of the *Retina* of one eye, and the nervous fibres on the right side of the *Retina* of the other eye, and so of those on the left; but no single object can be so painted in each eye, as to have its image on the right or left part of one *Retina*, that communicates with the right or left part of the other, of the same bigness, and at the same time as in the other; because, in whatever position the object is, it must be nearer to one eye than to the other, except it be just in a line from the nose between the two eyes, straight forwards.

Hence it is, that if there be two candles set before any person, the first at the distance of one foot, and the second at the distance of two feet from the eyes; he that looks at the second candle at *B* will see it single, but see the first candle, or the candle *A* double; one appearance being in the line *AD*, the other in *oAE*, because it paints itself upon *oo* in the *Retina* of each eye, which points are not the middle points, but farther from the nose than the middle points *mm*: So if *B* be the first candle, and *C* the second, he that looks at *B* will see *C* double, because it is painted in the *Retina* at the points *nn* nearer the nose than *mm*; and so will appear to be in the same position as *pr* in Fig. 27.

If $\gamma\phi$ be two candles dispos'd in such a manner, as in Fig. 30. that by the interposition of a perforated board FF, γ can paint itself only in the eye R, and ϕ in the eye L; upon making the optic axes to meet at B and tend towards ϕ and γ , ϕ and γ will each paint an image on the middle of the Retina of each eye, by crossing their rays at B; and thus the two candles will appear to be but one, or rather to be in one place, upon account of the communication of the middle of each Retina; but if instead of the candles, ϕ be a piece of red silk, and γ a piece of green silk, the same position of the eyes will make an image at B, appearing like a red and green spot together, without a mixture of the colours; if ϕ be a red hot iron and γ a candle of sulphur, the phenomenon will be more distinct; if the optic axes be turned directly towards γ and ϕ , as if there was no board FF in the way, there will appear two holes in the board, the one having the red hot iron in it, and the other the candle.

Now, if of the refracted rays of the candle in the first case, as in Fig. 25. those which diverge from each other, so as to fall into each eye, cause the same sensations respectively, as the rays which come from a red hot iron, and those which come from a blue candle; it is evident, that the candle in the first case affords red and blue rays after refraction, and that those rays are differently refrangible, as declining less from the perpendicular aa ; and the purple as bP , declining most from the said perpendicular: The same will, *ceteris paribus*, be found true in the intermediate rays; and in order to be assur'd, that the experiment is as Mr. Desaguliers has related it, the planes af and fd of the bar may be covered with paper.

An Account of what appear'd upon opening a big-bellied Woman near Haman in Shropshire; who was suppos'd to have continued several Years with Child; by Dr. Hollings. Phil. Transf. N^o 348. p. 452.

A Married woman near Haman, about three miles from Shrewsbury, and about the 40th year of her age, had then for the first time the common reasons to believe she was with child; at the time of her account she had the usual signs of labour, and a midwife assured her it was so; but that the child was so big, she could not be delivered without bringing it away in pieces; but not willing to submit to such an operation, her pains went soon off, and she continued without any other disorders nine months longer, when she had again the

signs of labour, and the same midwife assur'd her as before; but persisting in her former resolution, her pains, after a day or two, went off: Soon after her belly swell'd to a surprising size: The Dr. saw her first upwards of 20 years before, when her belly was almost even with her chin, and its weight so great, that she was oblig'd to support it with a stool; she could not stand without the help of a rope from the cieling, which assisted her in changing her posture of sitting; she slept commonly with her arms folded on her belly, and her head resting between them; she had no swelling in her legs; every other part being emaciated, as is usual in the like cases; thus she liv'd, without any other considerable complaint, upwards of 30 years, which the Dr. takes to be the most remarkable circumstance in her case: she died in May 1715, when this appear'd to be an *Ascites*.

So great a distension of the common integuments had distorted several of her ribs, and forced the diaphragm so high, that it was surprising to find her breathing could be continued so long: The water was all contained in the duplicature of the *Peritoneum*, which was 13 gallons and a quart; it was saltish with some little fat upon it, and towards the latter running it was tinged with blood as usual; there was no water in the cavity of the *Abdomen*, excepting what was contained in a kind of bladder, which lay a-cross the *Fundus Uteri*; this was divided by a cartilaginous substance into two cavities; in one there was a pint and a half, and in the other three quarters of a pint of water; this was what, as the Dr. takes it, impos'd on the midwife: The *Uterus* was of the natural size without any alteration, excepting that the *Os Tincæ* and *Collum minus* were fill'd with a gritty substance, hard as stone, which the Dr. takes to have been the humour, separated there, and coagulated by time: Mr. Cowper *Fig. 4. Tab. 15.* says, he found the same parts, fill'd with a glutinous matter; which, he supposes, is useful to prevent abortion; and if viriated, impregnation is hinder'd: The liver and other parts, contained in the *Abdomen*, were forced into an exceeding small compass (and by that pressure changed a little as to shape) to perform their office so long, to which the muscles of the *Abdomen*, distended so as to be scarce discernible, could give but little, if any, assistance.

The awe the vulgar have for dead bodies, tho' never so prejudicial to the living, would not suffer her friends to let the Dr. make any further enquiry; and the same error hinder'd

him

him from examining another woman, who died at the same place about a week after of an *Ascites* she had had for 40 years, any farther than to be satisfied, she had seven gallons of water contained between the duplicatures of the *Peritoneum*, and none in the cavity of the *Abdomen*.

A new Method of determining the Parallax of the Sun or his Distance from the Earth; by Dr. Halley. Phil. Trans. N° 348. P. 454. Translated from the Latin.

IT is well known, that this distance of the sun from the earth is suppos'd different by different Astronomers; *Ptolemy* and his followers, as also *Copernicus* and *Tycho Brahe* have computed it at 1200 semi-diameters of the earth, *Kepler* at almost 3500; *Riccioli* doubles this last distance, and *Hevelius* makes it only half as much; but at length it was found upon observing by the telescope, *Venus* and *Mercury* on the *Sun's* disk, divested of their borrow'd light, that the apparent diameters of the planets were much less than hitherto they had been suppos'd to be; and in particular, that *Venus's* semi-diameter, seen from the *Sun*, only subtends the fourth part of a minute, or fifteen seconds; and that *Mercury's* semi-diameter, at his mean distance from the *Sun*, is seen under an angle of 10 seconds only, and *Saturn's* semi-diameter under the same angle; and that the semi-diameter of *Jupiter*, the biggest of all the planets, subtends no more than the third part of a minute at the *Sun*; whence by analogy, some modern astronomers conclude that the *Earth's* semi-diameter, seen from the *Sun*, subtends a mean angle, between the greater of *Jupiter* and the lesser of *Saturn* and *Mercury*, and equal to that of *Venus*, viz. one of 15 seconds; and consequently, that the distance of the *Sun* from the *Earth* is almost 14000 semi-diameters of the latter: Another consideration has made these authors enlarge this distance a little more; for since the *Moon's* diameter is something more than a quarter of the *Earth's* diameter, if the *Sun's* parallax be suppos'd 15 seconds, the body of the *Moon* would be bigger than that of *Mercury*, to wit, a secondary planet bigger than a primary one, which seems repugnant to the regular proportion and symmetry of the mundane system; but on the contrary it seems hardly consistent with the same proportion, that *Venus*, an inferior planet, and without any satellite, should be bigger than our *Earth*, a superior planet, and accompanied with so remarkable a satellite; therefore, at a mean, supposing the *Earth's* semi-diameter, seen from the sun, or which is the same thing, the *Sun's* horizontal paral-

lax to be 12 seconds and a half, the *Moon* will be less than *Mercury*, and the *Earth* bigger than *Venus*, and the *Sun's* distance from the *Earth* come out nearly 16,500 semi-diameters of the *Earth*: The Dr. admits of this distance at present, till its precise quantity be made to appear more certain by the trial he proposes; nor does he regard the authority of such as set the *Sun* at an immensely greater distance, relying on the observations of a vibrating *Pendulum*, which are not, as it seems, accurate enough to determine such minute angles; at least, such as use this method will find the parallax sometimes none at all, and sometimes even negative; that is, the distance will become either infinite, or more than infinite, which is absurd; and it is scarce possible for any one certainly to determine by means of instruments, however nice, single seconds or even ten seconds; and therefore, it is not at all surprising, that the exceeding minuteness of such angles has hitherto baffled the many and ingenious attempts of artists.

As the Dr. was making his observations in the island of *St. Helena*, about 40 years before, on the stars round the south pole, he happened to observe, with the utmost care, *Mercury* passing under the *Sun's* disk; and contrary to his expectation, he very accurately obtained with a very good 24 foot telescope, the very moment in which *Mercury*, entering the *Sun's* limb, seemed to touch it internally, as also that of his going off; forming an angle of internal contact; whence he discovered the precise quantity of time the whole body of *Mercury* had then appear'd within the *Sun's* disk, and that without an error of one single second of time; for, the thread of solar light, intercepted between the obscure limb of the planet, and bright limb of the *Sun*, tho' exceedingly slender, affected his sight, and in the twinkling of an eye, both the indenture made on the *Sun's* limb by *Mercury* entering into it, vanished, and that made by his going off, appeared: Upon observing this he immediately concluded, that the *Sun's* parallax might be duly determined by such observations, if *Mercury* being nearer the *Earth*, had a greater parallax, when seen from the *Sun*; for, this difference of parallaxes is so very inconsiderable, as to be always less than the *Sun's* parallax, which is sought; consequently, tho' *Mercury* is to be frequently seen within the *Sun's* disk; he will scarcely be fit for the present purpose.

Therefore, there remains *Venus's* transit over the *Sun's* disk, whose parallax, being almost four times greater than that of the *Sun*, will cause very sensible differences between the times in which

which *Venus* shall seem to pass over the *Sun's* disk in different parts of our *Earth*: From these differences, duly observed, the Dr. affirms, the *Sun's* parallax may be determined, even to a small part of a second of time; and that without any other instruments than telescopes and good common clocks, and without any other qualifications in the observer than fidelity and diligence, with a little skill in astronomy; for, you need not be scrupulous in finding the latitude of the place, or in accurately determining the hours with respect to the meridian; it is sufficient, if the times be reckoned by clocks, truly corrected according to the revolutions of the heavens, from the total ingress of *Venus* below the *Sun's* disk to the beginning of her egress therefrom, when her opaque globe begins to touch the bright limb of the *Sun*; which times, as the Dr. found by experience, may be observed even to a single second of time.

But by the limited laws of motion, *Venus* is very rarely seen within the *Sun's* disk; and for a series of 120 years and upwards is not to be seen there once; that is, from 1639, when Mr. Horrox was favoured with this agreeable sight, and he the first and only one since the creation of the world, down to 1761; at which time, according to the theories, hitherto found agreeable to the heavens, *Venus* will pass under the *Sun* on May 26. in the morning; so that (vide *Phil. Trans.* N° 193.) at London, almost at six o'clock in the morning she is to be in the middle of the *Sun's* disk, and but four minutes more southerly than his center; the duration of this transit will be almost 8 hours; that is, from two till almost 10 o'clock in the morning; and consequently, her ingress will not be visible in *England*; for, the *Sun* at that time, being in 16° of *Gemini*, and almost in 23° of north declination, shall be seen not to set throughout the whole northern frigid zone; and consequently, the inhabitants of the coast of *Norway*, as far as its northern promontory, beyond the town of *Drontheim*, may observe *Venus* entering the *Sun's* disk; and perhaps this ingress into the sun at his rising may be seen by the inhabitants of the north of *Scotland* and those of *Zeland*; but when *Venus* is nearest the *Sun's* center, the sun will be vertical to the northern coasts of the gulf of *Ganga*, or rather of the kingdom of *Pegu*; and consequently, in the neighbouring countries, when the *Sun* shall, at the ingress of *Venus*, be almost four hours distant to the east, and almost as many to the west at her egress, her apparent motion within the *Sun's* disk will be accelerated almost twice as much as is the horizontal parallax of *Venus* from the *Sun*; because *Venus* at that time moves retrograde from east to

to west; whilst in the mean time an eye, on the surface of the earth, is carried the contrary way from west to east.

Supposing the *Sun's* parallax, as was said, to be 12 seconds and a half, *Venus's* parallax will be 43 seconds; and subtracting the *Sun's* parallax, there will remain half a minute at least for the horizontal parallax of *Venus* from the sun, and consequently, *Venus's* motion will be accelerated $\frac{1}{2}$ of a minute at least from that parallax, whilst she passes over the *Sun's* disk, in such elevations of the pole, as are near the *Tropic*; and still more so in the neighbourhood of the equator; for, *Venus* will at that time accurately enough describe within the *Sun's* disk four minutes an hour; and consequently, at least 11 minutes of time (by which the duration of this eclipse of *Venus* will be contracted by reason of the parallax) answer to $\frac{1}{2}$ of a minute; and by this contraction alone we might safely determine the parallax, provided the *Sun's* diameter and *Venus's* latitude were very accurately given, which yet we cannot possibly bring to a calculation, in a matter of such great subtilty.

Therefore, we must have another observation, if possible, in places where *Venus* possesses the middle of the *Sun* at midnight, under the opposite meridian, that is, 6^h or 90° more westerly than *London*, and where *Venus* enters the *Sun's* disk a little before his setting, and goes off a little after his rising; which will happen in the said meridian in about 56° of N. Lat. that is, at *Nelson's* harbour in *Hudson's Bay*; for, in the neighbouring places *Venus's* parallax will protract the duration of the transit, and make it at least six minutes longer; because whilst the *Sun* seems to tend under the pole from west to east, these places on the *Earth's* surface will seem to be carried with a contrary motion towards the west, that is, with a motion conspiring with the proper motion of *Venus*; consequently *Venus* will seem to move slower within the *Sun's* disk, and continue longer thereon.

If therefore, in both places this transit happen to be duly observed by proper persons, it is evident, that the *Mora* will be longer by 17 entire minutes in *Nelson's* harbour than in the *East-Indies*; nor does it matter much, whether the observation be made at *Fort St. George*, commonly called *Maderas*, or at *Bencoulon* on the western coast of the island of *Sumatra* near the equator; but if the *French* should incline to make the observation, *Pondecherry* on the western coast of the gulf of *Ganga*, at the elevation of 12° will be a proper place for that purpose; and for the *Dutch*, *Batavia* their famous *Emporium*, is a fit place: And, indeed, the Dr. would have several observations made of the

the same phenomenon in different parts, both for further confirmation, and least a single observer should happen to be disappointed by the intervention of clouds from seeing what the Dr. does not know if those either of the present or following age shall ever see again; and upon which, the certain and adequate solution of the noblest, and otherwise most difficult problem depends: Therefore, the Dr. again and again recommends it to the curious strenuously to apply themselves to this observation.

By this means, the *Sun's* parallax may be discovered, to within its five hundredth part, which, doubtless, will seem surprising to some; yet notwithstanding, if an accurate observation be had in both the places abovementioned, it has already been shewn, that the duration of these eclipses of *Venus* differ from each other by 17 entire minutes, upon the supposition, that the *Sun's* parallax is 12 seconds and a half; and if this difference be found to be greater or less by observation, the *Sun's* parallax will be greater or less almost in the same ratio; and since 17 minutes of time answer to 12 seconds and a half of the *Sun's* parallax; for each second of the parallax, there will arise a difference of upwards of 80 seconds of time; therefore, if this difference be had true within two seconds of time, the quantity of the *Sun's* parallax will be had to within the fortieth part of one second; and consequently, his distance will be determined to within its five hundredth part, at least, if the parallax be not found less than what the Dr. supposes it; for, 40×12 and $\frac{1}{2}$ is 500.

Here the Dr. has had no regard to the planet's latitude, both to avoid the trouble of a more intricate calculation, which would render the conclusion less evident, as also on account of the motion of the nodes of *Venus* not being hitherto discovered, and which cannot be duly determined but by such conjunctions of the planet with the *Sun* as this; for, it was only on the supposition, that the plane of *Venus's* orbit is immoveable in the sphere of the fixed stars, and that her nodes would continue in the same places wherein they were in 1639, that it was concluded that *Venus* would pass four minutes below the sun's center: And if in 1761 she should pass more southerly, it will be evident, that there is a regression of the nodes; but if more northerly, that there is a progression of them; and that at the rate of five minutes and $\frac{1}{2}$ in 100 Julian years, for each minute, by which the path of *Venus* will at that time be more or less distant from the *Sun's* center than the said four minutes; but the difference between the durations of these eclipses will be somewhat less than 17 minutes, by reason of the southern latitude of *Venus*; but greater if, by the

the progression of the nodes she shall pass over the *Sun* to the north of his center.

But for the sake of such as are not thoroughly acquainted with the doctrine of parallaxes, the Dr. further explains the matter both by a figure and a somewhat more accurate calculation: Therefore, supposing that at *London May 25. 17^h 55'* 1761, the *Sun* be in $15^{\circ} 37'$ of *Gemini*, and consequently, that at his centre, the ecliptic tends towards the north in an angle of $6^{\circ} 10'$; and that the visible path of *Venus* within the *Sun*'s disk does at that time descend towards the south, forming an angle with the ecliptic of $8^{\circ} 28'$; the path of *Venus* will tend a little towards the south in respect of the equator, intersecting the parallels of declination in an angle of $2^{\circ} 18'$: Supposing likewise, that *Venus* be near the *Sun*'s center at the said time, and distant therefrom towards the south four minutes, describing by a retrograde motion on the sun's disk four minutes an hour: The *Sun*'s semi-diameter will nearly be $15' 51''$, and that of *Venus* $37''$ and $\frac{1}{4}$; and supposing for trial sake, the difference of the horizontal parallaxes of *Venus* and the *Sun*, to be $31''$ such as it is on the supposition of the *Sun*'s parallax being $12''$ and $\frac{1}{4}$; therefore, let a small circle, as *ABD* Fig. 1. Plate VIII. be described from the center *C*, whose semi-diameter let be $31''$, representing the earth's disk, and therein drawing *DabE* and *cde* the ellipses of the parallels of 22 and 56 degrees N. Lat. in the same manner as is now used by astronomers for constructing solar eclipses, let *BCA* be the meridian wherein the sun is, to which let be inclined the right line *FHG*, representing the path of *Venus*, in an angle of $2^{\circ} 18'$, whose distance from the center *C* let be 240 parts whereof *BC* is 31 , and from *C* let fall the right line *CH* perpendicular upon *FG*; and supposing the planet in *H* at $17^h 55'$, or $5^h 55'$ in the morning, let the right line *FHG* be divided into the hourly spaces III. IV, IV. V, V. VI, &c. equal to *CH*, that is, four minutes; let the right line *KL* be also equal to the difference of the apparent semidiameters of the *Sun* and *Venus*, or $15' 13''$ and $\frac{1}{4}$; and the circle, described with the radius *KL* and from any point within the small circle, representing the *Earth*'s disk as a centre, will meet the right line *FG* in the point denoting what o'clock it is at *London*, when *Venus* shall touch the *Sun*'s limb in an angle of internal contact, in that place of the earth's superficies, that lies under the assumed point on the disk; and if a circle, described from the center *C* and with the radius *KL*, meet *FG* in the points *F* and *G*, the right lines *FH*, *HG* will be $= 14' 41''$, which *Venus* will appear to pass

pass over in $3^h 40'$; therefore, F will fall upon $2^h 15'$ at London, and G upon $9^h 35'$ in the morning; whence it is evident, that if the *Earth's* magnitude, should by reason of the immense distance, vanish, as it were, into a point; or if divested of its diurnal motion it should always have the sun vertical to the same point C, the entire *Mora* of this eclipse would continue for seven hours and $\frac{1}{2}$; but in the mean time whilst the *Earth* revolves with a contrary motion to that of *Venus* thro' 110 degrees of Long. and consequently, the duration of the said *Mora* is shorter, suppose 12 minutes, it will nearly be $7^h 8'$, or $107'$.

Now in the meridian itself *Venus* will be near the *Sun's* center at the eastern mouth of the *Ganges*, where the elevation of the pole is about 22° ; therefore, that place will be equally distant from the *Sun* on both hands, in the moments of the planet's ingress and egress, viz. 53° and $\frac{1}{2}$, as the points *a, b* in the greater parallel *DabE*; but the diameter *AB* will be to the distance *ab*, as the square of the radius, to the rectangle under the sines of 53° and $\frac{1}{2}$ and 68° that is, as $1' 02''$ is to $46'' 13'''$; and upon making a due calculation, the Dr. finds, that the circle, described with the radius *KL* from the center *a*, will meet the right line *FH* in the point *M*, at $2^h 20' 40''$, but described from the center *b* it will meet *HG* in *N* at $IX^h 29' 22''$ at London; consequently, the whole body of *Venus* will be seen from the banks of the *Ganges* within the *Sun's* disk for $7^h 8' 42''$; therefore, we have rightly supposed its duration $7^h 8'$ since here a part of a minute is inconsiderable.

But adapting the calculation to *Nelson's* harbour, the Dr. finds, that *Venus* shall pass under the *Sun's* disk, when he is just about to set, and emerge out of his disk immediately after his rising, that place in the mean time being carried thro' the hemisphere opposite to the *Sun* from *c* to *d*, with a motion conspiring with that of *Venus*; therefore, the *Mora* of *Venus* within the *Sun's* disk will become longer by reason of the parallax, suppose by four minutes, so as entirely to be $7^h 24'$ or $111'$ of the equator; and since the latitude of the place is 56 degrees, it will be, as the square of the radius to the rectangle under the sines of 55° and $\frac{1}{2}$ and 34° , so is *AB* = $1' 2''$ to *cd* = $28'' 33'''$, and upon duly making the calculation, it will appear, that the circle, described from the center *c* with the radius *KL* will meet the right line *FH* in *O* at $2^h 12' 45''$, but described from the center *d* it will meet *HG* in *P* at $IX^h 36' 37''$; wherefore, the duration of the *Mora* at *Nelson's* harbour will be $7^h 23' 52''$, to wit, greater than at the mouth of the *Ganges* by $15' 10''$ of

time: But if *Venus* should pass without latitude, the said difference will become $18' 40''$; but if she shall be four minutes more northerly than the *Sun's* center, the difference will be increased to $21' 40''$, and will be still greater by increasing the planet's N. Lat.

From the above hypotheses it follows, that at *London* *Venus* shall rise entering into the *Sun*, and at $9^h 37'$ in the morning in her egress touch internally the *Sun's* limb, and quite leave his disk not before $9^h 56'$.

It is evident from the same hypotheses, that *Venus* should touch with her center the extreme northern limb of the *Sun* on *May 23. 11^h 1769*, so that, by reason of the parallax, her whole body may be seen in the northern parts of *Norway*, within the *Sun's* disk; whilst on the coast of *Peru* and *Chili* she shall seem to ride on the disk of the setting *Sun* with a small segment of her body; as in like manner in the *Molucca* islands, and neighbouring parts, at sun-rising: But if the nodes of *Venus* be found to have a retrocession (as there is reason to suspect from some latter observations) then her whole body being every where seen within the *Sun's* disk, the greatest difference of these eclipses will afford a still more evident proof of the *Sun's* parallax.

An Account of the Cause of Venus being seen in the day time in 1716; by Dr. Halley. Phil. Trans. N^o 349. p. 466.

IT may justly be reckoned one of the principal uses of the mathematics, that in several cases they can prevent the superstition of the unskilful vulgar; and by shewing the genuine causes of uncommon appearances, deliver them from the vain apprehensions they are apt to entertain of what they call *prodigies*; of this kind was the appearance of *Venus* in 1716 in the day time; generally taken notice of about *London* and elsewhere, and by some reckoned to be *prodigious*: This put the Dr. upon enquiring how this planet should be so plainly seen by day; whereas she rarely shews herself so, unless to such as know exactly where to look for her: To resolve this, the following problem arose, viz. *To find the situation of the planet in respect of the earth, when the area of the illuminated part of her disk, is a Maximum.*

To investigate this *Maximum*, the Dr. assumed the following *Lemmata*. 1. That the visible area's of the disk of the same planet, at different distances, are always reciprocally as the squares of those distances, which is evident from the first principles of *Optics*. 2. That the area of the whole disk of the planet

is to the area of the illuminated part thereof, as the diameter of a circle to the versed sine of the exterior angle at the planet, in the triangle at whose angles are the sun, earth and planet.

3. That in all plain triangles, four times the rectangle of the sides, containing any angle, is to the excess of the square of the sum of the sides above the square of the base, as the diameter is to the versed sine of the complement of the contained angle to a semicircle, which the Dr. calls the exterior angle: This is a new theorem, and of good use in trigonometry, and easily proved from the 12, and 13. of the second book of *Euclid's Elements*.

This premised, putting m for the distance of the sun and earth, and n for that of the sun and *Venus*, and x for the distance of the earth and *Venus*, or the third side of the triangle sought; by the 3. *Lemma* $4nx$ will be to the excess of the square of $n+x$ above the square of m , as the area of the whole disk of *Venus* to the area of the part illuminated; and by the 1. *Lemma*, the area's of her whole disk are at all times as the squares of n reciprocally; whence the quantity $\frac{nn + 2nx + xx - mm}{4nx^2}$

will in all cases be proportional to the area of the illuminated part.

Now that this should be a *Maximum*, it is required, that the fluxion thereof be equal to 0, or that the negative parts thereof be equal to the affirmative, that is, that

$$2nx + 2xx \times 4nx^2 = 12nx^2 \times x \times nn + 2nx + xx - mm;$$

and dividing all by $4nx^2$, the equation becomes $2nx + 2xx = 3nn + 6nx + 3xx - 3mm$; consequently, $3nn + 4nx + xx = 3mm$, and therefore; $x = \sqrt{3mm + nn - 2n}$.

From hence a ready, and elegant geometrical construction becomes obvious; for, with the center S (as in Fig. 2. Plate VIII.) and radius $ST = m$ describe the semicircle TDA ; and with the same center and radius $SE = n$, the semicircle $EV B$, which two semicircles shall represent the orbs of the earth and *Venus*; then make the chord AD equal to the radius ST , and from D towards A lay off $DE = SE$; draw TF , and thereon place $FG = BC = 2n$, and with the center T and radius TC describe the arch GV , cutting the semicircle BVE in V , and draw the lines SV , TV ; the triangle STV is similar to that, at whose angles are the sun, earth and *Venus*, at the time when the area of the enlightened part of that planet's disk, as seen from the earth, is greatest; how this geometrical effect follows from the equation is too evident to need repetition.

In consequence of this solution, the Dr. finds this *Maximum* always to happen, when the planet is about 40 degrees distant from the sun; and the times thereof, about the middle between her greatest elongations on both sides from him, and her retrograde conjunctions with him; when little more than a quarter of her visible disk is luminous, and resembling the moon when about five days old; and notwithstanding her diameter is at that time but 50 seconds, yet she shines with so strong a beam, as to surpass the united light of all the fixed stars that appear with her, and cast a very strong shade on the horizontal plane on which they all shine; an irrefragable argument to prove, that the disks of the fixed stars are inconceivably small, and next to nothing; since shining with a native light, so many of them do not equal the reflex light of a quarter of a disk, less than one minute in diameter.

In this situation *Venus* was found on the 10th of *July* 1716; about which time, when the sun grew low, she was very plainly seen in the day time, for several days together; as she might have been in the mornings, about the latter end of *September*; but this, arising from the causes already shewn, is nothing uncommon; for, every eighth year it returns again, so that the planet may be seen on the same day of the month and hour, and very nearly in the same place; as all acquainted with the heavenly motions know.

Lastly, it is to be observed, that the equation $x = \sqrt{3mm + nn} - 2n$ has a limit; for, if n be equal to $\frac{1}{2}m$, the point V will fall on B ; and the whole disk of a planet at that distance from the sun would be the *Maximum*, viz. when in its superior conjunction with the sun; and the like if n were less than $\frac{1}{2}m$, the arch GV in such a case not intersecting the semi-circle BE .

An Account of a very uncommon subsiding of the Earth, near Folkestone in Kent; by Mr. Sackett. Phil. Trans. N° 349. p. 469.

FIG. 3. Plate VIII. represents a strait road from what is called the *mooring-rock* to *Tarlingham-house*; the country, as to the rising and falling, is much the same, for about a mile on each hand of the said road.

A represents the *mooring-rock*, about half way between high and low water-mark; B the foot of the cliff, 30 yards from the rock; C the top of the cliff about six yards high; CD a plain of 30 yards; DE a craggy cliff 60 yards high; EF a plain above a mile

a mile long; *B G* a hill of steep ascent, near half a mile; *C H* the land from the top of the hill to the house, near a mile; *I Tarlingham-house* lying near two miles and a half *N. N. W.* from the rock; *E G H* a line of sight; *K B L* the shore at high-water mark.

The *mooring-rock* (tho' it lies surrounded with a great many other rocks) is a very noted one, known by this name, time out of mind; here vessels use to be moored, while they are loading with the other rocks, which they take from hence for pier-heads; and a great quantity of them was shipped in the time of *Oliver's* usurpation, and carried to *Dunkirk* for the service of that harbour.

This rock has remained fixed in this manner in the memory of man; and old men have observed, that for 40 years and upwards, the distance between it and the foot of the lesser cliff *A B* has been much the same; tho' there seem nothing extraordinary in this, yet it is what they take special notice of, to their great surprise; for, they say, and prove by good marks, that the lesser cliff *B C* has been constantly falling in; insomuch, that from time to time, in their memory, near 10 roods forward to the land have been carried away by the sea; from whence, as it appears, that the plain between the top of the lesser cliff, and the foot of the higher cliff *C D* has formerly been double the breadth it is at present, so the distance between the rock and the foot of the lesser or lower cliff *A B* should have increased in proportion, and would have been double at present to what it has been formerly; but this distance remaining the same or rather less (as several think) is what is very surprising; nor can it be accounted for otherwise than by supposing, that the land pressing forwards into the sea is washed away by the high tides; and as often as this happens, presses forwards again; this pressing forwards of the land into the sea is plain matter of fact, and that not only at this one place of observation, but by like observations all along this coast, as far as the situation continues the same.

At the top of the higher craggy cliff at the point *E*, it is to be observed, that (as old men inform us) upwards of 40 years ago, not so much as the top of *Tarlingham-house* could be discerned, neither from hence, nor yet a pretty distance off at sea; but it discovered itself gradually, till at this day not only the whole house, but a great tract of land below it is plainly to be seen, as in the line of vision *E G H*; the tracts of land is more in proportion than what is described in the figure between the point at *H* and the house; in this there can be no fallacy; and

we

we can ascribe it to nothing less than the sinking of the hills; for, their tops could never wear away considerably, being always covered with grass, and never broken up by the plough or otherwise. These hills are all of chalk, and have probably very large caverns within side, springs of water always flowing plentifully from the foot of them; and Mr. Sackette was informed, that several cracks have been often observed on their tops; whatever be the cause thereof, it is not to be doubted, but that these hills are greatly sunk; and their subsiding, as the people at this place suppose, forces the cliffs and all the land forwards into the sea. The cliffs consist of large ragged sand-stones till we come to near a yard (at some places more) of the bottom; then we meet with what they call a *slipe*, i. e. a slippery sort of clay always wet; upon this *slipe* at the bottom they suppose, that the hard stony land above slides forwards towards the sea.

Several Observations made about Rome, Naples, &c. in 1683, 1684; by Dr. Tancred Robinson. Phil. Trans. N^o 349. P. 473.

IN his journey from Rome to Naples the Dr. observed on the rubbish of the *Tre taberne* an unusual vegetable for that place, remote from any town or house, viz. the *Ficus indica spinosa*, commonly called the *Opuntia*, or *Tuna*, and by our naturalists of America the *prickly-pear*, whose juice tinges the urine red; when he came to Naples, he found it there near the rocks, and in some wild solitary places, like a native; when the Spaniards plant it, they chuse desert places: The *cochineel-vermiculus*, before it changes into the *chrysalis* or *aurelia* of a lady-cow, is said to feed in great numbers on this plant; but the colour lies in the *nymph-worm* before it turns a *beetle*; from this the Dr. takes occasion to reflect upon the several species of our European worms, some of which, if tried, might be found to yield rich colours; we are assured, that the maggot of our *ilix* yields the *kermes*, and a noble scarlet dye before it turns into a fly; several shell-fish, which are a sort of insects, contain purple juices: In passing the *Appenines* and *Alps* the Dr. observed in some *Sirars* or beds, and even in the midst of the hardest rocks, great variety of perfect shells, that never occurred to him on the Italian shores, nor in any of the great number of *Museums* of that country; so he conjectures they might be exotic: Going farther on the *Via Appia* he observed a great deal of the *siliqua arbor* or *carob tree*, commonly called *panis S. Joannis Baptiste*, on the pulp of which several poor people were feeding; the husks seemed

to the Dr. to taste like *manna*; and near them grew plentifully the *arbor judæ*: The *arbutus*, or *strawberry-tree*, was common in the woody places; if this grow wild in the south west parts of *Ireland*, as some affirm, the Dr. supposes them much warmer than any county in *England*. Before he entered the beautiful *Campania* of *Naples*, large woods of cork-trees grew on each side the road, where the inhabitants were peeling them; and upon asking, whether the trees did not perish, they answered, some did, but that the acorns afforded new annual supplies; the women and children wore shoes made of the bark.

Coming near *Capua* the Dr. observed a species of *ornus* or ash, on whose trunk several saccharine concretions were to be seen; this proved the true *manna* that issues out thro' the incisions made in this tree by the inhabitants of *Calabria*; swarms of *cicada's* were sucking the body and boughs, and perhaps by wounding them made way for fresh *manna*: Here the Dr. observes, that several insects have not only a *proboscis* to bore and extract the juices of plants for aliment, but other proper instruments to convey their eggs into vegetables and animals, where they may find shelter and food when they come to hatch in the gall-tumours, and other excrescencies, occasioned by the wounds of the parent insects, that make such variety of *cuniculi* in all parts of plants, and even in the cutaneous parts of living and dead animals: This confirms the Dr. that several gums and exudations find their way out of vegetables, by means of the wounds of insects and other apertures: Most travellers thro' the *East Indies* affirm, that *gum-lack* is worked and made by large ants that cover the trees; the Dr. rather supposes, that the insects suck and terebrate the tree, and so give vent to that peculiar sap, that hardens in the sun: This may extend to most balsamiferous, gummiferous and saccharine plants, especially in hot climates where insects abound, and are more active: In cold climates the sap of several vegetables will boil into sugars, as that of maple, birch, reeds, &c. not but that the fluids of plants, like those of animals, will spontaneously break thro' their vessels in a *plethora*, and form on the superficies various eruptions and concretions: Here the Dr. takes occasion to observe, that there are many adulterations of *manna*; all passes for the *Calabrian*, whereas that of *Briançon* is from the *larix*, that of *Persia* from the *myrica*, and these frequently mixed with the juices of spurge, and other purgative ingredients: The Dr. acknowledges, that dew will sometimes in cool mornings shoot, and congeal into a solid, sweet, white substance,

france, which he once observed in very hot weather before sunrise.

Upon viewing the *Vulcano's* about *Naples*, *Vesuvius* on the east side, the *Solfatara* and *Monte di Cinere* on the west near *Puzzuolo* and *Baia*, the Dr. observed the same face of nature, which he supposes run thro' all the other *Vulcano's* of our globe, viz. heaps of pumice-stones and cinders of *Marchasites* on their sides, with beds of flowers of sulphur on their tops; the holes and cavities in those calcined minerals seem to be the *nidus* of the sulphur, which hath been sublimed by the heat and fire of that vast mass of *pyrites*, that compose the bowels of those *Vulcano's*, and lie scattered thro' several parts of the earth, even under the sea, where they sometimes germinate, ferment and take fire, throwing up little islands; so that earthquakes and other shocks of the globe may arise from the mines of these combustible and explosive minerals, laden with sulphur and elastic salts; hence, some account may be given of *Thermæ* or hot baths, whose waters gliding thro' these hot beds partake of their gas, or spirit; of these medicinal boiling waters and stoves, there are more about *Naples* than in any place the Dr. ever saw or heard of, the whole country being continually pervaded by hot steams.

Walking round this city the Dr. found palm-trees; some with unripe dates hanging down, others without any fruit; and there is another species of palm that sweats out the *Gum Dragon*; the Dr. supposes, that the monks transplanted them from *Africk*; here he saw growing several sugar-canes, rice, maiz, and a great deal of the purging senna and cummin seed: Thro' the whole *Campania* of *Naples* he observed the same vegetables to be larger and ranker than in any other parts of *Italy*, as the *Plantanus*, the *Lantiscus*, the *Terebinthus*, the *Pistaches*, the *Oleanders*, *Agnus castus*, *Barba Jovis*, the *Tragacanth*, the *Syrax*, the capers, &c. the melons, jujubes, azoxoles and other fruits were of a better taste; the *Gossypium*, with the cotton breaking out of the husks, adorned some of the fields; the hedges were full of pomegranates, almonds, tamarisk, sumach, cedrus, *Lycia* (a sort of juniper or savin) and a great deal of *Phillyrea*, *Alaternus*, *Cisti*, *Cyrtis*, *Myrtles*, spanish broom, bays, *Saurustines*, &c. all wild and natives of that warm soil and kindly climate: The water-lemons, the olives, the oranges, lemons and citrons were better than those about *Genoa* or in *Provence*: The *Lotus arbor* or nettle tree, the *Paliurus* or Christ-thorn, the *ricinus* or *Palma Christi* are common in the hedges, together with several *Thymelæa's*. The

The Dr. saw them fishing for coral and *hippotampi*; the first did not come soft out of the sea; a hard crust covers the vegetable part that bears seed, as the *alga's* and *fuci* do; they catch the *sword-fish* by darting a spear into him, as they do the whales in the *Groenland* fishery.

When dark night came on, the Dr. observ'd vast numbers of luminous flies thro' the *Campania* of *Naples*; perhaps our male glow-worm, or flying *cicindela* may abound there; not but that several other insects may carry such lanthorns about them; the scorpions creep out about that time; and the Dr. has found them often in bed with the *punaises*, or bugs. The hedges are full of lizzards of various colours, and the *cicada's* chirp and sing towards evening; he observ'd several species of stinging spiders in the corn-fields; some of which, in hot harvests, may prove *tarantula's*, the poison of animals and plants increasing with the approach of the sun and the heat of the climate; a great many silk-worms were spinning on the trees and shrubs; the birds prey'd upon them, before they could change into *papilio's*, as they do on swarms of locusts: The Dr. often eat their young frogs, tortoises and snails, serv'd up with oil and pepper, as also their *sea-urchins* and the *Urtica marina*, call'd sea-gelly or blubber, tho' it be an animal, with a true heart and vessels for the circulation of fluids; some of their thistles are no ungrateful faller.

He saw some vitriol works about *Siena*, *Rome* and *Puzzuolo*; and alum works only about *Civita Vecchia*; amongst the sands of the *Adriatic* sea he observ'd several white, clear, shining flints; which, the Dr. was told, were carried to *Venice* to make the fine chrystal glass at *Muran*.

The magnificent *septizonium Severi*, as represented by Dr. *Musgrave* in his *Geta Britan. & Synop. Chronol. Dom. Sever.* stood near the foot of the *Palatine* hill, on the E. S. E. side, overlooking the *Via Appia* and *Circus maximus*, *Titus's* amphitheatre being near on the other side; by the number of *portico's*, which were seven, it might contain a vast many people, as spectators of the triumphal entries, and publick games.

Dr. *Musgrave* takes it to be the *Sepulchretum* or burying place of that imperial family; tho' most of the ancient *Mausoleums*, at least such as Dr. *Robinson* saw, were *Rotonda's* or *Columbaria's*, for the more convenient placing the urns of the kindred; as that of *Augustus* near the *Campus Martius*; that of *Adrian* on the opposite bank of the *Tyber*; those said to be *Scipio's*, *Cicero's* and *Munatius Plancus's*, near *Capra* and the

Via Appia; that of *Virgil* on the side of mount *Pausilippus*; that of *C. Metella* and some others on the *Via Flaminia*; some were pyramidal as that of *Cestius* in the wall of *Rome*, and a few others on the public roads: This *septrizonium Severi* seems to differ from the rest of these ancient *Sepulchreta*, or private burying-places; which might be varied according to the fancy and humour of great families: This urn-burial was only in fashion amongst the better sort, as for the dead bodies of the plebeians and slaves, they were generally laid in places, where they had dug stone, and these quarries became catacombs; the laws prohibited them to bury within a city, unless the bodies were first reduced to ashes.

The Dr. observ'd, in several of the ruins both about *Rome* and *Naples*, large stones laid close together, and wedged very fast with little or no cement; the bricks towards the middle of a building were generally of a rhomboidal figure, very smooth, shining and hard, laid in plaister as firm as marble; their mortar was much more durable than ours, as appears at this day by their *Aqueducts* and *Piscina's*, the *Cento Camere*, and *Caligula's* bridge under water at *Baia*; *Pliny* says they made use of the *terra Puteolana*; but the present inhabitants have lost the way of tempering it.

During his stay at *Genoa*, *Leghorn*, *Ostia* and *Civita Vecchia*, the Dr. saw several torpedo's or cramp fishes, very accurately dissected by *S. Lorenzini*; and he also saw a great many *sphyræna's* (a species of sea-pike, a-kin to the needle fish) the *uranascopeus*, call'd *bocca* in *Capo* and *Prete*; the *mola* or sun-fish; the *dentex* or *pentalis*; *altavela's*, a sort of *pastinaca*; the *pesce balestra* or *capricus*; the *pesce pettine* or *novacula*; the *xygæna* or balance-fish, as large as the saw-fish or most sharks; the *scolopax* or *trombetta*, call'd by our seamen the bellows or trumpet-fish; the *draco marinus*; the tunny-fish; the *centrina*, or *pesce porco*; the *aquila*, the *scorpius major*, with a variety of *turdi* in the markets; but what pleas'd the Dr. most, were some odd sea-animals, as the *lepus marinus*, a species of naked snail; the *bystrix marinus* or *eruca*, call'd by the seamen *pincio*, with a brush hanging out of the tail, resembling the *byssus* or silk of the *pinna*; several *tamburo's* or drum-fishes; and a plenty of *murena's*; he also observ'd a strange sea-animal call'd the *microcosmo marino*, with several shells, *tubuli* and vegetables growing or sticking to its back; this appear'd to the Dr. a-kin to the *echini marini*, or rather to the *stella marina*, being triangular, and sometimes pentadactylous.

The Dr. went out in a boat with the fishermen, who shew'd him several *loligo's*, *polypi*, and *sepia's* or cuttle-fish, all crustaceous; some of them were casting out their ink into the water; because, as the Dr. supposes, some sharks, dog-fish or other enemies were near them; this black liquor may probably be the gall of those animals; in their nets the Dr. often found sea insects and vegetables; and indeed a new world, undescrib'd by naturalists, at least unknown to him; so that a great many things escap'd him, and were utterly lost for want of the art of drawing; therefore, the Dr. would advise all travellers to be conversant in that very useful science.

He observ'd, that the *Italians*, near the *Alps* and *Appenines*, call'd several birds *francolino's*, as our red, grey and black game; and even their own red and white partridges; the different colours of the hens from the cocks, the several variegations in their feathers, the different ages and places, have all given occasion to multiply names and species; and the same thing may happen in fishes, quadrupeds, insects and all the divisions of *Zoology*, and even in *Botany* and *Minerology*.

The *Italians* call several of their little fat birds *beccafigo's*, that feed upon figs, grapes, and other sweet fruits; thus the *French* multiply their *ortulans* taken in the vineyards and gardens: Some of the ancient authors observe, that the *Romans* us'd to feed their geese and other birds with figs, when they intended to swell their livers to a very great size: The *merops* or *apiafter* is common on their brooks; it flies like our *Kingsfisher*, and preys not only upon insects but fish; there is a very beautiful bird in *Italy*, that hangs its nest down from the boughs of trees; when the Dr. saw it fly by him, he took it for an *Indian* bird, from the brightness of its colours; it is as large as our missel-bird and thrush. *Quer. an Icterus Plinii?* The great cock of the wood, said to be found in *Ireland*, is common on the sides of the *Italian* hills, and frequently brought to the markets: He saw twice or thrice the *Himantopus*, and the *Phenicopterus* or *Flamingo*, whose tongue was a dainty amongst the *Romans*, when they grew luxurious: He also saw some spoon-bills; these three last birds were wading in the rivers and marshes near the sea; he once spied *Peleicans* on the *Adriatic*, near the mouth of the *Po*; the *Avis Diomedea* was hung up dried in one of the *Museum's* at *Florence*, but they told him, it had been taken on some of the isles of the *Archipelago*.

On the *Laguna* of *Venice*, the Dr. saw several species of *Mergi*, *Lari*, *Colymbi*, and other water fowls, most of which div'd; he was surpris'd with the variety of them, having never seen so many on other coasts; perhaps the hard winter had forced some unusual birds thither; the Monks and Fryars told the Dr. they eat some of these sea-birds in *Lent*, and on fast days, because they liv'd upon fish, and had a fishy taste, as the *French* pretend their *Macreuse* have, which is a kind of sea-duck, common on the coast of *Normandy*, and brought to the markets, even at *Paris* on *Maigre* days, of which vide an account in *Phil. Trans.* N^o 172.

Buffalo's are common in the kingdom of *Naples*, and in some parts of *Lombardy*, where they plow and draw with them; a peculiar cheese is made of their milk, call'd *Casio di Cavallo*, rould up like stiff pieces of ribbon; of their black shining horns they make snuff-boxes and combs; these animals are unruly, and therefore they lead them with iron or brass rings, drawn thro' their noses; they make buff-leather of their skins; he once saw some hairy sheep feeding on a common, which were possibly brought from *Afric*.

In passing the high *Alps*, the Dr. had a view of the *ibex* or *Steinbock*, whose large horns are recurvated almost as far back as the tail; they are very ponderous for the bulk of the animal, having several knotty rings, that may help them in climbing; they are rarely caught: The *rupicapra* or *chamois* is very common on the sides of the cliffs, whose skins afford soft leather; the *Mus Alpinus* or *Marmota* is as large as a rabbit, will soon grow tame in houses, tho' brought down from the summits of the highest mountains, where it will grow fat.

The Dr. saw in several towns of *Italy*, fresh strong porcupines, which the inhabitants told him were taken in the hedges and ditches thereabouts, tho' much more rare than our land *Urchins*: In the country of the *Grisons*, and in some cantons of *Switzerland* he has often observ'd the *ranunculus veridis* or small tree-frog, perching on the boughs and leaves: In the northern parts of *Germany* he saw several elk-skins, and those of the rein-deer stuffed, and set up in *Museum's*, but never alive; tho' these animals are said to be common in *Muscovy* and *Lapland*, and sometimes seen in the forests of *Prussia*: The skins of the *hippopotami*, said to be the *Behemoth*, are in some collections of curiosities in *Italy* and *Holland*, so likewise are those of the musk-deer, one of which is in the *Museum* of the *Royal Society*.

As to that *Aurora borealis*, whose phenomena are describ'd and explained in *Phil. Transf.* N^o 347. the Dr. is of opinion, that those phosphorous or luminous appearances in the firmament proceed from the various effluvia, perspir'd out of our globe, or passing thro' it; for, he has seen those lights over *Vesuvius*, the *Strombulo* islands, and towards *Ætna* in dark nights, when those *Vulcano's* were not flaming nor burning, their sides and tops being passable to travellers at that time, and all their external parts quiet: We are assur'd, that *Iceland* and *Groenland* abound in *Vulcano's*; so likewise may north-east *Lapland*, north *Russia* and *Tartary*, where vast chains of mountains are said to run; the *Jesuits* and other travellers relate several prodigious eruptions of fires and earthquakes towards the north of *China*, but nearer the pole, the earth must be clos'd and pent up for several months, by the long severe frosts, and continual snow and ice, which relaxing towards spring, may give vent to that vast mass or magazine of perspirable matter, that had been confined so long in hot subterraneous prisons; and this may be one reason, why animal bodies themselves are often sensible of changes at that season in our climate; when perspiration is upon such an increase.

An Account of the mischiefs ensuing the swallowing of Stones of Bullace and Sloes; by Mr. Derham. Phil. Transf. N^o 349. p. 484.

AMONG the several accounts transmitted to the *Royal Society* of the mischiefs ensuing the swallowing of divers sorts of stones, there is no case, wherein the lesser stones of fruit, as sloes and bullace in particular, have produced any dangerous symptoms, especially in the stomach alone; the larger stones of prunes and plumbs have produced, it is true, very fatal effects; but many swallow the lesser stones of sloes, cherries, &c. rather out of choice, than with any apprehensions of danger, supposing them useful in preventing a surfeit from the fruit; but the following case will shew the danger even of these: The man-servant of a neighbouring clergyman complained to Mr. *Derham* of excessive pains in and about his stomach, and that whatever he eat he could not retain it, but in a little time throw it up again; by which means he was reduced to a very low and languishing condition, insomuch that his life began to be despair'd of: Upon this, applying himself to some practitioners in physick, one of them plied him with strong

strong vomits for eight days together, with very little signs of success; but some time after having occasion to ride somewhat more than ordinary, he found himself much pained in his stomach, and sick, which ending in violent vomiting and straining, brought up the first stones he ever perceived to come from him, about 20 in number: After this he had frequent returns of the vomiting up of bullace and sloe-stones, especially upon violent exercises, particularly upon moving and stooping, as also in riding, tho' gently: Upon these occasions he would be seis'd with acute pains in his stomach, and soon after he would throw up more of those stones; he has reckoned upwards of 120 bullace and sloe-stones, besides several others that came up when he was riding, or about his business; but after he had discharged them, he was much easier for a while; he commonly brought up a slimy matter with them, mix'd with blood or something very like it.

The cause of all this disorder, the patient ascrib'd to his greedily devouring all sorts of fruit he could come at, and particularly bullace and sloes, which he us'd to swallow down in great quantities, without evacuating many of the stones by stool, as he observ'd others did: These stones, he thinks, lay in his stomach, at least some of them, upwards of ten years; but he felt no pain till about four years before; and at first not so violent, nor attended with such severe fits of vomiting and loss of appetite, as afterwards.

Observations and Experiments relating to the Motion of the Sap in Vegetables; by Mr. Bradley. Phil. Tran. N^o 349. P. 486.

PLANTS, in general, are either terrestrial, amphibious or aquatic; and so nearly do vegetables agree with animals in most particulars, excepting local motion and its consequences, that from the knowledge of the one we are reasonably led to the discovery of the other: Those plants, which he calls terrestrial, are such trees, shrubs and herbs, as grow only on the land, and these, like land-animals, have diversities of food, method of generating and certain periods of life: Of the amphibious race, which live as well on land as in the waters, are willows, rushes, mints, &c. and these in many respects are not unlike the otter, tortoise, frog, &c. The aquatics, whether of lakes, rivers or the sea, are very numerous; and these may be compar'd with the fish-kind, and, like them, will not live out of their proper element; in fresh waters are the
water

water lillies, plantains, &c. and in the sea, corals, *fuci*, &c. Plants seem to possess only the next degree of life below the most stupid animal; or where animal life ceases, there the vegetable life seems to begin: The seasons of motion in plants are the same with those of animals, which sleep during the winter: An artificial heat will give motion to either of these in the coldest season.

The common opinions as to the motion of the sap, are as follows; first, the sap does not rise by the pith, because some have observ'd the trunks of large trees to be without that part, and yet the same trees have continued to put forth fruit, and branches on their tops: Mr. *Bradley* observ'd, that the pith is not found in those branches of a tree which exceed two or three years growth; and it is certain, that the pith which is in a branch of this year, for instance, will (the greatest part thereof) be distributed into those boughs which form themselves the next season.

It is said by some, that the tree does not receive its nourishment by the bark; for that trees, after losing that part, will still continue their growth: Others tell us, that if the bark be cut away round the trunk of a tree, it will presently dye: These various opinions seem to have been set on foot without extraordinary consideration, and on the persuasion that a tree has but one bark; whereas, upon examining with the microscope, we find four distinct coverings to each branch without the woody parts; the two outermost barks may be taken from a tree without great damage, but the other two, which lie nearer the wood, being stripped off, will kill the tree.

Some affirm, that the sap neither rises nor falls in the woody part of a tree, because they have not been able to discern any sap to issue out of that part, when a branch has been cut: The microscope plainly shews us the vessels in the wood, thro' which the sap rises from the root; but as these tubes are not large enough to admit into them anything grosser than vapour, so they have been esteemed to be of no great use; but the explanation of the following figure will in some measure discover their office, and that of such other parts of a plant, as are severally designed for its growth; but in the first place it is proper to enquire a little into the nature of the root.

The root of a tree is chiefly compos'd of a *Parenchyma*, more gross than that in the stem or body of the tree; it has likewise vessels and a covering; the root, that is, the principal part of it,

it, receives into it such juices of the earth as are proper for it, and no other; in some measure resembling a wick of cotton, which, being impregnated with oil, will only admit oil into it; this provision being made in the stomach of the plant; as it may be called, chiefly in the autumnal months, the tree is prepared for germination so soon as the earth is sufficiently warmed, either by the sun's beams, or an artificial heat, such as horse-dung, bran and water, or other the like ferments; these heats raise into vapour the juices contained in the root, and by that means cause vegetation.

Fig. 4, Plate VIII. is part of the branch of an apple-tree, formed in *May* 1715, and cut in *April* 1716, and that in the figure of a half cylinder, the length being somewhat more than the diameter, which was about a quarter of an inch; this being magnified with one of *Campani's* microscopes, discovers the following parts; viz. 1, 2, 3, 4, 5, 6, 7, capillary vessels, which run longitudinally thro' the branch in the woody part, formed in 1715; thro' these tubes the steam rises from the root, the strength of which is well explained by *Capt. Savory's* engine for raising water by fire; from A to B we may view vessels of the same sort, formed at the same time; 8, 9, are vessels of the same use with the former, and that formed themselves for 1716; by this means the diameter of the branch is increased, and additional nourishment suffer'd to pass into those buds, which are to make new branches; and these are made out of the fourth or innermost bark, mark'd C C; the mouths of the capillary tubes of the years 1715, 1716, are D E; the vapour which rises from the root, is continued in these vessels to the extremities of the branches, where it meets with parts (not here represented) resembling glands; which glands, if they may be so called, are likewise found at every knor or joint; at these places the vapour coming near the air is condensed, and returns between the barks, by means of its own weight, down F G H, leaving in each bark, mark'd I K L, such juices as each of them is naturally adapted to separate from it; till at last the more oily part, passing to the root, may lengthen the fibres thereof; as icicles are lengthened; and by its oily particles preserve them from rotting by the wet; the parts which compose the several barks are parenchymous or spongy; the first mark'd M is of a closer contexture than the second N, and the second closer than the third O, and so on, till these parenchymous parts are interwoven with the longitudinal wood-vessels where they are somewhat contracted,

till they come to form the pith, marked P; then they are much larger than in any other part of the tree; and by what Mr. Bradley has observed, seem to contain a more finished juice than the rest, and may properly enough be called the *medulla*.

We may observe, that when the 4th or innermost bark C has once compleated its sap-vessels, and is firmly joined to the woody part, then the third bark O takes its place for the succeeding year; and so of the rest, only that the first, marked M, splits and divides itself to supply the place of the second.

Mr. Bradley recommends the following enquiry to the curious; viz. if the several barks, having a different contexture of parts, admit into each separate and different juices from the rest, whether those juices may not be of different virtues; the first more astringent than the others, the second perhaps emetic, and the third cathartic.

Some Microscopical Observations on the vegetation and exceeding quick propagation of moldiness on the Substance of a Melon; by the Same. Phil. Trans. N^o 349. p. 490.

MR. Bradley had a large melon, which he split lengthwise thro' the middle, in order to observe the vessels, which composed the membrane or tunic of each *Ovarium*; but laying by one half of the melon, at the end of four days, he found several spots of moldiness began to appear on the fleshy or pulpy part of the fruit, somewhat green towards the rind, and of a paler colour towards the middle of the fruit; these spots grew larger every hour, for the space of five days, at which time the whole fruit was quite covered over.

This surprising vegetation made Mr. Bradley curious to examine, if there were any difference between those parts which were green and the others, besides their colour; the first, viewed with the microscope, appeared to be a *fungus*, as in Fig. 5. Pl. VIII. whose cap was filled with about 500 small seeds, which shed themselves in two minutes after they had been in the glasses: The other sort had several leaves resembling grass, among which appeared some stalks with fruit on their top; each plant might well enough be compared to a sort of bull-rush as in Fig. 6. they had great quantities of seed, which he supposes were not above three hours before they began to vegetate; and it was about six hours more, before the plants were quite perfected; for, about seven o'clock one morning, he found three plants at some distance from any others, and about four o'clock in the afternoon the same day, he could discern upwards of 500 more grow-

growing in a cluster with them, which he supposed were seedling-plants of that day; the seed of all these were then ripe and falling.

When the whole fruit had been thus covered with mold for six days, this vegetable quality began to abate, and was quite gone in two days more; then was the fruit putrified, and its fleshy parts now yielded no other than a stinking water, which began to have a gentle motion on its surface, that continued for two days without any other appearance: He found then several small maggots, as Fig. 7. move therein, which grew for the space of six days, after which they laid themselves up in their bags; thus they remained for two days more without motion, and then came forth in the shape of flies, as Fig. 8. the water at that time was all evaporated, and there remained no more of the fruit than the seeds, the vessels which composed the tunics of the *Ovaria*, the outer rind, and the excrement of the maggots; all which together weighed about an ounce; so that there was lost of the first weight of the fruit when it was cut, upwards of 20 ounces.

From this and other cases of the like nature we may judge, how much vegetable life depends on fermentation, and animal life on putrifaction.

Diving improved; by Dr. Halley. Phil. Trans. N^o 349.
P. 492.

THERE have been several methods proposed, and engines contrived, for enabling men to continue a competent time under water; and the respiring fresh air being found to be absolutely necessary to maintain life, several ways have been thought of for conveying this *pabulum vitæ* down to the diver, who must necessarily, without being some how supplied therewith, return very soon or perish.

The *Divers* for sponges in the *Archipelago*, help themselves by carrying down in their mouths sponges dipped in oil; but considering how small a quantity of air can be supposed to be contained in the pores or interstices of a sponge, and how much that little quantity will be contracted by the pressure of the incumbent water, it cannot be supposed, that a supply, obtained by this means can long subsist a diver; since it has been found by experiment, that a gallon of air, included in a bladder, and by a pipe reciprocally inspired and expired by the lungs of a man, will, in little more than a minute of time, become unfit for any farther respiration; and tho' its elasticity be but little altered, yet in passing the lungs, it loses its vivifying spirit, and is rendered

dered effete, not unlike the medium found in damps; which is present death to those that breathe it, and which in an instant extinguishes the brightest flame, or the shining of glowing coals or red hot iron, if put therein: The Dr. leaves others to determine what it is the air loses by being taken into the lungs, or what it communicates to the blood by the extreme ramifications of the *aspera Arteria*, so intimately interwoven with the capillary blood-vessels; as also to explain how it is performed; for, hitherto there has been no discovery made to prove, that the ultimate branches of the veins and arteries there have any *Anastomoses* with those of the *Trachæa*, as they are found by the microscope to have with each other; and only concludes from the aforesaid experiment, that a naked diver without a sponge may not be above a couple of minutes under water (as the Dr. saw a *Florida Indian* at *Bermudas*) nor much longer with a sponge, without being suffocated, and not near so long without long use and practice; ordinary persons generally beginning to be stifled in about half a minute of time; besides, if the depth be considerable, the pressure of the water on the vessels is found by experience to make the eyes blood-shot, and frequently to occasion spitting of blood.

When, therefore, there has been occasion to continue long at the bottom, some have contrived double flexible pipes, to circulate air down into a cavity, inclosing the diver, as an armour, to bear off this pressure of the water, and to suffer his breast to dilate upon inspiration; the fresh air being forced down by one of the pipes with bellows or otherwise, and returning again by the other pipe, not unlike an artery and vein; this, it is true, has been found sufficient for small depths, not exceeding 12 or 15 feet; but when the depth surpasses three fathoms, experience teaches us, that this method becomes impracticable; for, tho' the pipes and the rest of the *Apparatus* may be contrived to perform their office duly, yet the water, its weight, being now become considerable, does so closely embrace and clasp the limbs that are bare, or armed with a flexible covering, that it obstructs the circulation of the blood in them, and presses with so much force upon all the junctures, where the armour is made tight with leather, skins, &c. that if there be the least defect in any of them, the whole engine will instantly fill with water, which will rush in with so much violence, as to endanger the life of the diver, who may be drowned before he can be drawn up; upon both which accounts the danger increases with the depth; besides, a man thus shut up in a weighty case, as this must needs be, can-

not but be very unweildy and unactive, and therefore, unfit to execute what he is designed to do at the bottom.

To remedy these inconveniencies the *diving bell* was next thought of, wherein the diver is safely conveyed into any reasonable depth, and may stay a longer or shorter time under water, according as the bell is of a greater or lesser capacity: This is most conveniently made in form of a truncated cone, the smaller basis being close, and the larger open, and to be so poised with lead and so suspended, as that the vessel may sink full of air with its open or larger basis downwards, and as near as possible, in a position parallel to the horizon, so as to close with the surface of the water all at once: Under this receptacle the diver sinks down together with the included air into the depth desired; and if the cavity of the vessel contain a tun of water, a single man may remain therein at least an hour, without much inconvenience, at five or six fathoms depth; but this included air, as it descends lower, contracts itself according to the weight of the water that compresses it; so as that at 33 foot deep or thereabouts, the bell will be half full of water, the pressure thereof being then equal to that of the whole atmosphere; and at all other depths, the space possessed by the compressed air in the upper part of the bell will be to the under part of its capacity filled with water, as 33 feet to the depth of the surface of the water in the bell below the common surface thereof; and this condensed air, being taken in with the breath, soon insinuates itself into all the cavities of the body, and has no sensible effect, if the bell be permitted to descend so slowly as to allow time for that purpose; the only inconvenience that attends it is found in the ears, within which there are cavities opening only outwards, and that by pores so small, as not to give admission even to the air itself, unless dilated by a considerable force: Hence on the first descent of the bell a pressure begins to be felt on each ear, which by degrees grows painful, as if a quill were forcibly thrust into the hole of the ear; till at length the force overcoming the obstacle, that which constringes these pores yields to the pressure, and letting some condensed air slip in, present ease ensues; but the bell descending still lower, the pain is renewed and again eased after the same manner: On the contrary, when the engine is drawn up again, the condensed air finds a much easier passage out of those cavities and even without pain; this force on the auditory passages might possibly be suspected to be prejudicial to the organs of hearing, but experience teaches the contrary; but what is more inconvenient in this engine is the water entering into it, so as to contract the bulk of

of air, according to the aforesaid rule, into so small a space, as that it soon heats and becomes unfit for respiration, for which reason it must be often drawn up to recruit it again; and besides, the diver, being almost covered with the water thus entering into his receptacle, will not be long able to endure the cold thereof.

To obviate these difficulties, which attend the use of the common diving-bell, the Dr. contrived means to convey air down to it, whilst below; whereby not only the included air was renewed and recruited, but also the water entirely driven out, at whatever depth it happened to be; and this he effected by an easy contrivance, which may furnish air at the bottom of the sea in any quantity desired; the description of the *apparatus* is as follows.

The bell, he made use of, was of wood, containing about 60 cubic feet in its concavity, and of the form of a truncated cone, whose diameter at top was three feet, and at the bottom five: This he coated with so much lead, as that it would sink empty, and he distributed the weight in such a manner about its bottom, that it would go down in a perpendicular position and no other; in the top he fixed a strong but clear glass, as a window to let in the light from above; and likewise a cock to let out the hot air that had been breathed; and below, about a yard under the bell, he placed a stage; which hung by three ropes, each of which was charged with about 100 weight, to keep it steady; this machine he suspended from the mast of a ship, by a sprit, which was sufficiently secured by stays to the mast-head, and was directed by braces to carry it over board, clear of the ship side, and to bring it again within board as occasion required.

To supply air to this bell, when under water, he caused a couple of barrels, of about 36 gallons each, to be cased with lead, so as to sink empty; each having a bung hole in its lowest part to let in the water, as the air in them condensed on their descent, and to let it out again, when they were drawn up full from below; and to a hole in the uppermost part of these barrels he fixed a leathern trunk or hose, well liquored with bees-wax and oil, and long enough to fall below the bung-hole, kept down by a weight appended; so that the air in the upper part of the barrels could not escape, unless the lower ends of these hose were first lifted up.

The air-barrels being thus prepared, he fitted them with tackle proper to make them rise and fall alternately, after the manner of two buckets in a well; which was done with so much ease, that two men with less than half their strength, could perform all the labour required; and in their descent they were directed by lines, fastened

fastened to the under edge of the bell, which passed thro' rings, placed on both sides the leathern hose in each barrel; so that sliding down by those lines, they came readily to the hand of a man, who stood on the stage on purpose to receive them, and to take up the ends of the hose into the bell; thro' these hose, as soon as their ends came above the surface of the water in the barrels, all the air included in their upper parts was blown with great force into the bell, whilst the water entered at the bung holes below, and filled them; and as soon as the air of the one barrel had been thus received, upon a signal given, that was drawn up, and at the same time the other descended; and by an alternate succession furnished air so quick, and in so great plenty, that the Dr. himself had been one of five who had been together at the bottom, in 9 or 10 fathoms water, for above an hour and a half at a time, without any sort of bad consequence; and he might have continued there as long as he pleased, for any thing that appeared to the contrary; besides, the whole cavity of the bell was kept entirely free from water; so that he sat on a bench, diametrically placed near the bottom, being quite dressed with all his cloaths on: He only observed, that it was necessary to be let down gradually at first, as about 12 foot at a time, and then to stop and drive out the water that entered, by receiving three or four barrels of fresh air, before he descended farther; but being arrived at the depth designed, he then let out as much of the hot air, that had been breathed, as each barrel would replenish with cold air, by means of the cock at the top of the bell; thro' whose aperture, tho' very small, the air would rush with so much violence, as to make the surfaces of the sea boil, and cover it with a white foam, notwithstanding the great weight of water over him.

Thus the Dr. found he could do any thing that was required to be done just under him; and that, by taking off the stage, he could, for a space as wide as the circuit of the bell, lay the bottom of the sea so far dry, as not to be over shoes thereon; and by the glass window, so much light was transmitted, that, when the sea was clear, and especially when the sun shone, he could see perfectly well either to read or write, much more to fasten or lay hold on any thing under him that was to be taken up; and by the return of the air-barrels, he has often sent up orders, written with an iron pen on small plates of lead, directing how to move him from place to place, as occasion required; at other times when the water was troubled and
thick,

thick, it would be below as dark as night; but in such a case, he could keep a candle burning in the bell, as long as he pleased, notwithstanding the great expence of air requisite to maintain flame.

The Dr. takes this to be an invention applicable to various uses; such as fishing for pearl, diving for coral, sponges, &c. in far greater depths than has hitherto been thought possible; as also for the preparing and levelling the foundations of moles, bridges, &c. upon rocky bottoms, and for the cleaning and scrubbing of ship bottoms when foul, in calm weather at sea: By an additional contrivance the Dr. found it not impracticable for a diver to go out of the engine to a pretty distance from it, the air being conveyed to him in a continued stream by small flexible pipes, which may serve as a clue to direct him back again, when he would return to the bell.

Observations on the Glands in the Human Spleen, and on a fracture in the upper Part of the Thigh-bone; by Dr. James Douglass. Phil. Trans. N^o 349. p. 499.

THAT anatomy, as well as physic and surgery, has received much improvement from a careful and true observation of what was found in dissecting morbid bodies, will appear from the two following instances, amongst several others that might be adduced for that purpose; for, it is certain, that nothing has contributed so much towards forming a right notion of the nature of the several diseases, and a true knowledge of the structure of many parts of the human body, as their appearance in a preternatural state, as will appear from the following instance; in which the glands were visible to the naked eye, and appeared dispersed thro' the fibrous substance of the human spleen; the subject was a boy of about four or five years of age, that died of a general atrophy, or consumption of all the muscular fleshy parts of the body, occasioned, undoubtedly, from the many glandulous swellings scattered up and down the whole mesentery; which, by compressing the lymphatic vessels, called in this place *vasa lactea*, prevented the access and supply of the chyle, so necessary for the continued nourishment and increase of the parts; for, without the constant recruit of this whitish balsamic liquor, the mass of blood will in a short time be unfit to perform any of those offices, which a fresh accession of chyle qualifies it for.

In a piece of this spleen, might be seen, without a glass, several round whitish bodies of a pretty hard consistence, and
a great

a great many small white and softer specks; but both of the same nature: These appeared to the Dr. to be so many distinct glands become visible, which in a natural state are only to be seen by a fine glass, as the curious *Malpighi* first observed in his treatise *de Liene cap. 5. de quibusdam corporibus per liem dispersis*: 'These very small glands, says he, are not so readily found in the spleens of all animals; it is true, that by only tearing the spleen of an ox, sheep, &c. they appear; but in a man they are not seen so easily: Yet if the entire system of the glands be preternaturally swelled, by this increase of their bulk they become more visible, as I once observed in a girl, in whom the whole spleen abounded with clusters of globules.' This case was the same with that the Dr. mentions.

The second observation the Dr. makes is about the nature of a luxation of the head of the thigh bone; for, by a careful examination of the part in the dead body, the common mistake of the writers of surgery now extant was discovered, and what was reckoned and treated by them as a luxation of the head of the *femur*, was found to be nothing other than a fracture of the same bone, near its neck, the globular head being still retained close in its socket, called *acetabulum coxendicis*.

Amongst all the writers of surgery and anatomy, there were only three who were apprised of this mistake; the first was *Ambrose Paree*, the second Dr. *Ruyseh*, and then Mr. *Chefelden*: And considering the depth of the articulation; the surprising strength of the muscles that surround it; the several strong ligaments that bind the head within the socket; the smallness of the neck of the bone; its porous and spongy substance, which makes it much weaker than the rest; and lastly, the disadvantageous oblique position of this neck, which exposes it the more to external accidents; from these and such like considerations, the Dr. says it will plainly appear, that a fracture in that part from an external cause, can much more easily happen than a dislocation.

An old woman turned of fourscore fell from her chair, and thereby had a fracture in the upper part of the thigh bone; she lived three weeks after it; and tho' it never was reduced, yet she complained of little or no pain, which may seem very extraordinary: It is observable, that the fracture was not only oblique, near the neck of the bone, but that each *trochanter*, i. e. the two processes near its neck were likewise broken off; and both drawn up almost as high as the head of the bone

hope itself, by the strong contraction of the *glutei* and other muscles.

Observations on some of the primary Planets, and particularly the occultation of a fixt Star by Jupiter; by Mr. James Pound. Phil. Transf. N^o 350. p. 506. Translated from the Latin.

Without accurate observations, it would be in vain to expect true theories of the celestial motions, and calculations agreeing with the heavens; that astronomy has made no greater progress is owing to this, that the ancients, and especially *Ptolemy*, have handed down to us no observations, besides such as they made use of as principles to establish their own hypotheses and tables; tho' it had been much more to the purpose to have faithfully transmitted the observations of *Timocharides*, *Aristillus*, *Hipparchus* and *Ptolemy*, and the disagreeing of their numbers with the heavens (after the example of the great *Hippocrates*, who, did not at all blush to consign to posterity both those who died, and those who recovered under his hands) rather than with a kind of vain glory, to have concealed those errors, they themselves were aware of, by concealing the observations, by which they found their tables did not agree with the heavens; but before *Tycho Brahe*, this was a common failing of almost all astronomers.

But after that the true theory of the motion of the planets was discovered by the sagacious *Kepler*, and its truth geometrically demonstrated by the great *Sir Isaac Newton*, some were eager of exhibiting astronomical calculations, entirely agreeing with the heavens; and for that end, not only the observers of princes, but several private astronomers here in *England*, were very diligent in observing the stars; among whom was *Mr. Pound*, who made the following observations at *Wansted*, with the greatest accuracy, by means of very long telescopes and a micrometer.

In 1715, August 21. 8^h 25' and $\frac{1}{2}$ equated time, *Mars* preceded, in right ascension, the middle star of the forehead of *Scorpio* (*Bayer's* δ) 6' 54", and was more northerly 9' 47".

September 18. 7^h 30' *Mars* preceded the bright star in the foot of *Serpentarius* (*Bayer's* θ) 17' 48", and had the same declination accurately.

November 30. 18^h 8', *Saturn* preceded γ or the second star in the arm-pit of *Virgo* 23' 19", and was more southerly 25' 3". But on December 4. 17^h 25' *Saturn* preceded it 10' 30", and was more southerly 29'.

But in 1716. February 22. 7^h 23' equated time *Mars* preceded ζ of *Pisces*, or the following of the three brighter stars in the south *linum* of *Pisces* 3' 35", and was more southerly 1' 23", which therefore it should cover in less than two hours, and perhaps with its body.

June 22. 8^h 52', equated time *Venus* followed *Cor Leonis* 34' 50", and was more southerly 7' 23".

August 14. 15^h *Jupiter* preceded *Propus* only 1' with not quite 14' 26" N. declination.

August 19. 13^h 2' *Jupiter* preceded the telescopic star, marked *b* 50' 8", with precisely the same declination.

August 24. 12^h 19' *Jupiter* was distant by the micrometer from the said *b* 5' 54", and at the same time from another brighter star *a* 7' 17"; the distance of these from each other was 12' 31", and at that time the lesser diameter of *Jupiter* was 38".

September 12. 17^h *Venus*, just after her second station, followed the telescopic star 17' 40", and was more southerly 5' 30"; but this fixed star was at that time in 27° 44' and $\frac{1}{2}$ of α and 5° 39'. S. Lat.

October 15. 17^h 12' and $\frac{1}{2}$ *Venus* was distant by the micrometer from the fixed star τ in *Leo's* leg 27' 55".

November 20. 6^h 18' and $\frac{1}{2}$ *Jupiter* had a regression towards the stars *a* and *b*, at which he was observed August 24. and was distant from *b* 6' 21", but from *a* 11' 36".

November 21. 7^h 38' *Jupiter* was distant from *b* 9' 19", and from *a* 3' 48", and *a* and *b* were distant from each 12' 30"; *Jupiter's* lesser diameter or axis was 44", and afterwards at 18^h 50' the star *a* was seen to adhere, as it were, to *Jupiter's* limb, and to be more northerly than his center about $\frac{1}{2}$ of his semi-diameter or 15"; but according to these observations it appears, that the middle of the occultation of the fixed star by the interposition of *Jupiter's* body happened November 21. 19^h 55', or nearly so.

November 30. 5^h 41' *Jupiter* preceded *Propus* 12' 36", and was more southerly 7' 36".

December 4. 6^h *Jupiter* followed *Propus* 22' 49", and was more southerly 7' 47".

December 5. 6^h *Jupiter* followed *Propus* 31' 35", and was more southerly 7' 50".

December 6. 6^h *Jupiter* followed *Propus* 40' 30", and was more southerly 7' 52".

December 7. 6^h *Jupiter* followed *Propus* 49' 15", and was more southerly 7' 54".

From

From these last observations it appears, that *Jupiter* and *Procyon* had the same Long. December 1. $15^{\text{h}} 29'$, at which time *Jupiter* was more southerly than that star $7^{\circ} 40'$; and from the same observations it will also appear that *Jupiter* was in opposition to the sun, as to Long. December 6. $12^{\text{h}} 46'$.

It is to be observed, that those telescopic stars, marked *a* and *b* are in Mr. *Flamsteed's Catalogus Britannicus*, where a place is assigned to *a* in the beginning of 1690 in $27^{\circ} 54' 29''$ of *Gemini* with $21^{\circ} 55''$ S. Lat. and to the other *b* in $28^{\circ} 5' 24''$ of *Gemini* with $28^{\circ} 5''$ S. Lat. And we know of no other fixed star covered by *Jupiter's* body, and observed since the invention of the telescope, but the said star *a*; to which *Jupiter* was very closely applied upwards of 83 years before, viz. in 1633 on the 9. of December in the evening N. S. when at *Digne in France*, *Gassendus* saw *Jupiter* in conjunction with this star, and but five semidiameters of his body above it; whence by a due calculation, it will appear, that the nodes of this planet, and the plane of its orbit are either immoveable or at least move but very slowly, in the sphere of the fixed stars. vide *Gassendi Obs.* Tom. 4. p. 162.

A Description of the Wood-pecker's Tongue; by Mr. Waller. Phil. Trans. N^o 350. p. 509.

THE *picus martius* or wood-pecker has several particularities in the structure and mechanism of its whole body; all which are wisely contrived and adapted, either for the catching the food of the individual, or conservation of the species.

That this bird makes a round hole even in sound and hard trees, as oak, horn-beam, beech, &c. is commonly observed; and that within these holes, the hollow being enlarged, the nest is made, the eggs laid and hatched, and the young brood fed, as by other birds is no less certain: For this purpose, the muscles of his neck, breast and thighs, are exceeding strong in proportion to the bigness of the bird; he has also a very firm strong sharp bill; his legs are strengthened with very strong tendons; and his toes, which are two before and two behind (as in some other birds) are provided with sharp strong hooked claws or talons; besides this, his tail consists of 10 very stiff, large and strong quills, firmly set into a strong *uropygium* or rump; so that when he has fastened his claws and feet into the clefts and inequalities of the bark of the tree, he claps his strong tail-feathers to the body of the tree, and so stands with his head erect to give the strokes with his bill with the greater force.

It is certain that this bird is of the insectivorous kind; and that he lives not only upon insects creeping on the outside of trees; but also on such as are under the bark between that and the wood, as likewise on those in rotten wood, as on worms and other insects in the ground; for, Mr. Waller has frequently observ'd the roots of their bills very dirty, as is the case with crows and rooks, &c. whence he supposes, he strikes his long sharp bill into the soft earth, thence to take out the worms; he has also found their craws full of small ants.

But the mechanism of the tongue of this bird is very curious; it is known to exert a long slender round tongue, to a considerable distance beyond the extremity of its bill; and to retract it again with great quickness into the mouth or bill, with the caught insect spitted on its tip: The *Chameleon*, it is true, darts out its tongue to a considerable length; and after entangling the fly in the glutinous matter at its extremity retracts both it and the prey into its mouth; but the mechanism in that animal is wholly different from that of the present subject, as may be seen in the account towards a history of animals in the *Memoirs of the Royal Academy*: The protrusion, therefore, of the tongue of this bird, for 3 or 4 inches being very extraordinary, and the mechanism of the several parts for that end, no less curious, several learned and diligent enquirers into nature have attempted to explain it; but Mr. Waller is of opinion, that they have been mistaken, at least in some particulars.

That learned and curious enquirer M. *Perault* describes it thus, in his *Essais de Physique*, Tom. 3. part 2. p. 148. this long tongue he exerts by means of two small bony cartilages, about seven inches long, and each of the thickness of a middling pin, and perfectly smooth; these two cartilages are united at the extremity, and, being in this place cover'd with flesh, form the fore-part of the tongue; the rest of these cartilages are separated from each other, and pass winding under the ears; and then rising up behind the head, where they meet again, pass over the top of the head, and so extend themselves to the root of the beak; the cartilages, which form the hinder part of the tongue, are also inclos'd in a channel, fleshy on the outside, and cover'd with a very smooth membrane on the inside.

Now these fleshy channels, which encompass these cartilages, are the muscles, by which the tongue is mov'd; for, having their origin at the *larynx*, and their insertion at the extremities of the cartilages, it comes to pass, that when these muscles

cles of the two fleshy channels, which form the hinder part of the tongue, are shortened, they force the fore-part of the tongue out of the beak, by drawing the posterior or farthest end nearer the *larynx*; and on the contrary, when the fleshy channel, which forms the anterior part, acts, it draws the fore-part of the tongue into the bill towards the *larynx*: This mechanism of making a hard part, such as bony cartilages are, to come out and return into another, such as the canals are, by means of cords, which are the muscles, is made use of in pulling up the glasses of the doors in coaches; for, the string being fastened to the lower part of the glass-frame, makes it rise when drawn, which resembles that action of the muscles, by which the tongue is mov'd.

M. *Perauld* gives the figures of these cartilages and other parts, as also of the head of the bird: Either the wood-peckers in *France* are different from ours in *England*, or this figure of the head is very ill designed, it being much too broad and large, and the beak too short; besides, he makes the two cartilages come to the root of the beak separately, one on one side, and the other on the other; whereas, in all the wood-peckers heads Mr. *Waller* had met with, the two cartilages join close together about the top of the head, and thence proceed joined, tho' not fastened to each other, slanting a little towards the right nostril, where they terminate together.

Besides, upon viewing and examining several subjects, Mr. *Waller* could not find them agree in many particulars with M. *Perauld's* account and explication; for, the muscles, which are fastened to the end of the cartilages at the root of the upper beak, are not inserted at the *larynx*, but pass on and are fastened to the lower bill; this pair Mr. *Waller* takes to be the muscles chiefly concerned in forcing the tongue out of the bill; there is another pair of muscles, which, being fastened to the place where the two bony cartilages are articulated with one single bone in the fore-part of the tongue, is, as he supposes, the chief pair concerned in drawing the tongue with its prey into the mouth; these proceeding from that articulation of the cartilages, as far as the *larynx* (each of them sending a branch to the *cartilago scutiformis*) from thence go on with the neck, tho' not fastened to it, till they come within the cavity of the *thorax*, where they are inserted under the *clavicula*: This pair is represented by *qq*, Plate VIII. Fig. 9, and by *kk*, Fig. 10 there is likewise a very slender white thread (whether a tendon or nerve Mr. *Waller* cannot determine) which accompa-

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nies this muscle its whole length; and which, when drawn gently, for fear of breaking, pulls in with it the end of the tongue; as there is such an other all along the *vagina* to the end at c.

Volker Coiterus, as mentioned by *Gerard Blasius* in his *Anatom. Animal. cap. 24. p. 64.* treating of the tongue of this bird, makes it to consist of three slender bones, round, and, as he says, *invicem colligatis*, or bound together, which is a mistake; for, tho' you reckon the two bony cartilages as *officula*, yet the third is not bound up with them, but articulated to the end of them: The same author says, the tongue may be protruded to the length of an inch and a half; whereas, when retracted it is scarce half an inch long; when really it may be exerted near four inches; and Mr. *Waller* supposes, cannot be drawn in, so as to be less than an inch and a quarter, that is, to that place where the two cartilages are articulated with the single bone; besides, this author makes the use of the long flat muscle, running over the top of the head, to be to draw the tongue to the upper jaw; whereas, their use is to protrude the tongue out of the bird's mouth: But this author having given us no figures, has render'd what he says less intelligible; tho' it is true, he mentions two pair of muscles, as there are so many chiefly concerned; yet there are at least two other pair, that assist in the performance.

Alphonfus Borelli's account in his treatise *de motu Animalium* part 2. p. 24. is likewise unsatisfactory in several respects, and the figure he gives to explain it very defective, and ill designed: He makes the pair of muscles, concerned in protruding the tongue, to be fastened, as it is true they are, to the lower beak towards the point; but then he makes their insertion at their other end to be at the extremities of the two *ossa hyoidea*; whereas, they really reach to the very end of the long cartilages, that go round the head; these, by another mistake, he makes to be the *retractores lingue*, and joins another pair as co-partners in the same action, which he makes to be twisted spirally about the *trachea*; none of all which agree with the subjects Mr. *Waller* had met with.

In the history of the Royal Academy of Sciences, publish'd in *Latin* by M. *du Hamel* in 1698, lib. 4. cap. 5. there is another description of this surprising mechanism by M. *Mery*. In this he differs both from *Perault* and *Borelli*, taking the horny extremity and bone to which it is joined, to be only the tongue, properly so call'd; and that the next two bones answer the

the *hyoides*, with the long cartilages adhering to them; but even in this he seems to Mr. *Waller* not to be so clear, confounding the two bones with the cartilages: He describes the *vagina*, with which the bones and part of the cartilages are encompass'd, and which is fastened to the horny extremity, and exerted and retracted with the tongue; he takes notice that the small sharp points or prickles on the horny part are moveable, and with their points bend towards the throat; but Mr. *Waller* supposes it to be a mistake to make the mucous matter glutinous, which is furnish'd by the two pyramidal glands; for, he takes the chief, if not the only use of that *mucus* to be to lubricate the passage in the *vagina*, for the more easy slipping of the cartilages therein.

Mr. *Mery* describes the muscles for exerting the tongue, and extends them from their insertion at the lower beak to the end of the *springy ligaments*, as he terms what Mr. *Waller* calls cartilages; to which he adds another small ligament, capable of extension, at the end of the other two, next the nose, which when the tongue is exerted, is relax'd and stretch'd: He also, describes the pair of muscles, fastened to the root of the tongue and *os hyoides*, and serving to draw the tongue into the mouth; these he makes to wind round the *aspera Arteria* once or twice, in which Mr. *Waller* thinks there is some mistake; being of opinion, that the mechanism, for this action of drawing in the tongue, is different from what is here describ'd: But Mr. *Waller* proceeds to the explication of the several draughts made by himself, with all possible care and exactness, in eight or ten several subjects.

Fig. 9. represents the head with part of the neck of this bird, after taking off the skin; A the skull, with two shallow grooves or channels, or rather one broad groove, with a small eminence in the middle, on the *occiput* or back part, from each side of the neck to the top of the head, where they unite into one, which passes slanting towards the right side, and terminates at the hole for the nostril on that side at *a*; *b* is the passage for hearing; *d* a large white gland, containing a glutinous liquor, almost like cream, as to colour and consistence, which empties itself into the mouth, to lubricate, as Mr. *Waller* supposes, the cartilages; *e* the eye, which has a bony ring, encompassing the *Iris*; *f* part of the tongue, which, in this figure is represented, as drawn almost all into the mouth; *g* part of the neck, which is large and furnish'd with very strong muscles; *h* the *oesophagus*, opening very wide at the *fauces*.
and

and entirely muscular; *i, i, i*, a long, but thin and flat muscle in respect of its breadth, about one eighth of an inch, reaching from the end of the cartilage at *c* to the lower bill or beak at *k*, to the inside of which it is very firmly fastened; as is such another on the other side; *k* the lower bill very strong and sharp pointed, articulated with the scull a little behind the hole of the ear *b*; *l, l, l*, the cartilage on one side, the other being exactly the same; this cartilage is round, very smooth, even and slippery, about the size of a pretty large pin; and reaches, when the tongue is drawn in and the muscle *i, i* relaxed, from the root of the upper beak at *r* to the root of the tongue properly so called, or to the bones of the tongue, where they are articulated, being bent like a hoop, as represented in the figure, slipping very freely in a sheath or membranous duct, fastened on the outer or convex edge of the flat muscle *i, i, i*, which accompanies it from its end at *r*, almost to the end of the canal or sheath, which opens at a hole a little before the *larynx*; and thence the muscle proceeds to its insertion into the lower beak at *k*; from the concave edge of this muscle, there is a thin and transparent, but very strong membrane, braced like a drum-head to the scull at *m*, where it is very strongly fastened; this membrane is furnished with capillary veins and arteries, and doubtless, is nervous; *n, n* represents this membrane; this cartilage, when the tongue is exerted, divides about $\frac{1}{4}$ an inch from the root of the beak at *c*; *o, o* a pretty large vein and artery; *p, p* a muscle reaching from one jaw to the other under the throat, serving as a bandage to keep in the cartilages, the root and the *os hyoides* of the tongue, as it may be called, from its starting out at that part, where are the articulations of the cartilages with the bones, when by the muscles, inserted into the sheath at or near *p*, and thence passing to the end of the tongue, it is drawn into the mouth; *q, q* one of the last mentioned muscles, which is round, of the size in the figure, and fastened to the breast of the bird, cut off at *r*; *s* the *aspera Arteria* consisting of perfect rings; *t, t* a muscle accompanying the *aspera Arteria*.

Fig. 10. A, A represents the under part of the lower bill; B, *b* the tongue; *b* the place where the two cartilages and two bones, represented by *f, f* in Fig. 12. are brought into and inclosed in one membranous sheath; C, C two glands displaced in this Fig. *c, c* two muscles attending these glands, and fastened near the end of the bill; *d, d* the two bony cartilages, bent and passing on each side of the neck, but united at *b*; *eee, eee* a pair of muscles, one accompanying each cartilage from its end at the upper beak.

and firmly adhering to the *vagina*, in which it slips, till about *f, f*; *f, f* the place where these muscles leave the *vagina*, and pass on to the inside of the bill, where they are inserted; their office is to thrust the tongue forwards, or out of the mouth; *g, g* a pair of muscles fastened a little below the *larynx* to the muscular part of the *aspera arteria* at *i*; the other end of them going up to the place *b* at the root of the tongue, whence they go on, encompassed by the *vagina*, to the articulation of the cartilages with the two bones; Mr. Waller takes their action to be to draw the end of the tongue towards the *larynx*; *k, k* two muscles fastened at one end within the *thorax*, under the *clavicula*; and at the other ends to the articulation of the cartilages with the two bones of the tongue, marked *f, f* in Fig. 12. these have the abovementioned nerves accompanying them: Mr. Waller takes these to be chiefly concerned in drawing in the tongue; each of these sends a branch to the cartilage at the top of the *aspera arteria* at *n*; *l, l, l, l*, two muscles running along and fastened to the sides of the *aspera arteria* from the *thorax* to the place where they are united, and where each of them sends out a branch, which binding over the bones and cartilages tends to the *fauces*, where they are inserted; *m* part of the *gula*; *n* a cartilage at the top of the *aspera arteria*; *o, o* the *aspera arteria*; *p* the neck bending like an S; in this bird the wind-pipe and *gula* always pass on the right side of the neck.

Fig. 11. A, A represents the two long flat muscles, shewn by *i, i* in Fig. 9. these join close to each other at the top of the head, and so pass on together to the end of the cartilages, to which end, as Mr. Waller takes it, they are fastened; and from whence a slender weak kind of ligament reaches to, and is inserted at the right nostril, at the root of the upper beak; this ligament is relaxed when the tongue is exerted; *b, b* the cartilages running in their sheath on the outside of the said muscles; *c* the *larynx* or passage to the *aspera arteria*; Mr. Waller observed no *epiglottis*; *d, d* two articulations or joints in the lower beak or bill; *e* the hole or passage, by which the tongue in its sheath is exerted and drawn in again; *f* what Mr. Waller calls the tongue, in the inside of which the two cartilages are brought together, till they are both articulated to one single bone, at the end of which is the horny barbed tip; *g* one of the pyramidal glands; *h* the lower bill.

Fig. 12. A represents that part, which Mr. Waller thinks may most properly be called the tongue, a small bone running thro' it; this as far as *c* is flat and thin at the sides; it is cut away at

d to shew the bones within it; *b* the horny tip of the tongue, about a quarter of an inch long, strong and sharp, furnished with four or five barbs on each side (not with an infinite number as *Coiterus* affirms) these barbs are sharp and moveable, like the small teeth at the root of the tongue and beginning of the *gula* in the pike and jack-fish, in that of eagles, &c. so as to let the prey slip easily on, but not get off again so easily; *c* the end of the bone of the tongue, where the two bony cartilages are articulated; *d* the place where the upper part of the tongue is cut away to shew the bone; *e* several small tendons, or rather, as he takes them, nerves running thro' the tongue; of these some tend to the extremity of the cartilages, and others accompany the muscles to the neck; *f, f* two bones or cartilages, which, in this bird, are united by a thin membrane as far as the next joint, so as to open asunder to some distance, but not to separate quite; these two bones seem to answer to the *ossa hyoidea* in other animals; at the place marked *g, g* the muscle that draws the tongue into the mouth is fastened, or rather leaves the tongue at that place, having its insertion near the end thereof; this muscle is represented by *q, q* in Fig. 9. *h, h* the two bony and springy cartilages running on each side of the neck; and which being joined close together on the top of the head do pass thus united to the right nostril.

From the consideration of these four figures, and comparing them together, the true mechanism and motion of the tongue seems to be in short this; *viz.* the two long muscles inserted near the end of this lower beak, and reaching to the end of the cartilages, being contracted, the round hoop of the cartilages is drawn up from each side of the neck, close to the pyramidal glands; and at the same time the muscles that draw the tongue into the mouth being relaxed, and the articulations at *c* and *g, g* in Fig. 12. being brought nearly to a right line, the tongue is exerted to the length of four or five inches; but when these long muscles are relaxed, the pair of muscles represented by *k, k* in Fig. 10. being contracted, draw the articulations *g, g*, where they are fastened, down into the throat or wide loose skin of the neck; and at the same time the cartilages opening into a wide hoop, the whole tongue is drawn into the mouth.

Fig. 13. *A* represents the skull; *b* the shallow *crena* or groove for the cartilages; *c* the place of their termination at the right nostril; *d* the orbit of the eye; *e* the hole for the optic nerve; *f* a hole or passage quite thro' from one orbit to the other; *g* a bone covering the hole to the ear; *h* the lower jaw and bill; *i* a ridge

ridge or process in the scull, beginning at the root of the upper bill, and keeping the two ends of the bony cartilages in their place on the right side; *k* the *os jugale*; *l* the upper bill.

Fig. 14. represents the right leg and foot, in which there are two *digiti* before, and two behind; the strength, largeness and sharpness of the hooked claws or talons are remarkable.

Fig. 15. A represents the *oesophagus*; B the *ingluvies* or crop, partly muscular, and lined with a glandulous coat; this Mr. *Waller* found quite filled with small black pismires; as also C the *ventriculus* or gizzard, which joined close to the crop; *d, d, d* the intestines nearly of the same bigness for their whole length; *e* the beginning of the *rectum*; *f* the *pancreas*.

Fig. 16. represents one of the middle pair of feathers in the tail, in which the great strength of the quill for so small a feather, and its bifurcate end, are very remarkable.

Fig. 17. represents the roof of the mouth, where it is observable, that the *rima* or passage for the air to the nostrils is beset on each side with a row of 10 or 12 small sharp teeth, with their points standing inwards, towards the *gula*; these take the prey from the end of the tongue, whose barbs or prickles are moveable, and prevent its going out of the beak again with the tongue, and from hence it is conveyed to the swallow.

An Account of the Phœnicopterus or Flamingo; by Dr. James Douglass. Phil. Transf. N° 350. p. 525.

ARISTOPHANES the famous comedian is the first that makes mention of this bird by the name of *Φοινικωντερος* *Aves* sc. 4. and not long after, it is called *ὄρνις πορνικα* by *Philostratus* in his life of *Apollonius Tyanæus* lib. 8. p. 387 Edit. Paris. 1605. *Folio. Apicius, Pliny, Sueton. Juvenal* and other Latin authors retain the Greek word, and call it *Phœnicopterus*. *Bellonius Histoire des oiseaux* lib. 8. cap. 8. says, that in French, it is called *le Flement* or *Flambant*: *Scaliger* affirms that in Provence they call it *Flammant*: And *Gesner Hist. Animal. lib. 3.* says, that it may be called *Avis rubra per excellentiam*: *Aldrovandus Ornithol. Tom. 3. lib. 20. cap. 4.* writes, that in *Sardignia* it goes by the name of *Flamingo*; and *de Laet* tells us, the *Spaniards* in the *West-Indies* call it *Flamenca*: *Dr. Charterton* and *Dr. Grew* convert the Greek appellation into English, calling it the *Phœnicopter*: And *Sir Hans Sloane* in his catalogue of *Jamaica* birds, annexed to *Mr. Ray's Synopsis Avium* calls it the *Flamingo*: *Du Tertre Hist. des Isles, &c. p. 300.* calls it *le Flamand*, which differs but little from the name given it by

Bellonius: And to mention no more, *Du Hamel Hist. de l'Academie Roy. p. 213.* says, it is commonly called *Becharu* in France.

All these different names may easily be accounted for from the colour most predominant in its wings: Thus *Martial Epigram 58, lib. 3.* says of this bird.

Nomenque debet quæ rubentibus pennis.

And again *Epigr. 71, lib. 13.* he makes it give the true derivation of its own name.

Dat mihi penna rubens nomen.

The Greek name is compounded of two words, viz. *κόρινθος*, *punicus*, *ruber*, and *πτερόν*, *ala*, *quod sit rubentibus alis*, as having red wings; and this is differently expressed by the following authors; *Bellonius* says it is called in French *Flambant*, not only *à colore dactylorum*, from the date colour of its wings, i. e. a scarlet or light red, like the fruit of the palm or date-tree, called in Greek *κόρινθος*, but also from the lustre of the colour, resembling flame; or as *Aldrovandus* has it, *quod velut ignis instar ejus rubedo emicet*. The words of *Gesner* are to the following purpose; 'I suppose that it had its French name from the red and flame-like colour of its beak, legs, and wings in some parts; or perhaps, because it leaves Flanders in the winter for the sea coasts on the south of France; for the French call a Fleming *Flammant*; or from the tallness of its body, for which the people of Flanders are generally remarkable': Mr. *Willoughby Ornithol. lib. 3. sect. 2. cap. 1.* says, it has its name from the flame-like colour of its wings and feet, rather than that it comes in the winter from Flanders; for, he believes there was scarce ever a bird of that kind seen in Flanders; so far are they from being common there, and flying from thence into other countries: Dr. *Grew Museum Reg. Soc. p. 67.* takes its Greek name to have been given it from the scarlet colour of its wings, and *Flammant* in French for the same reason: *Du Hamel* explains its name *Becharu* by *aratri-rostrum*, *quasi bec-charrie*, i. e. plough-beak, *quoniam rostrum ejus aratri instar inflectitur*.

All authors from *Aristophanes* down to *Aldrovandus* have accounted the *Phœnicopterus* a bird of the palmipede or web-footed kind; and tho' this last mentioned author will not allow

it to be so, yet he acknowledges, that it is not a true *fissipede* or digitated fowl; his words are, *nam & membrana digitos sepe-entis quoddam habet rudimentum*: Dr. Charleton alone, among all the later naturalists, has approved of his division, and accordingly ranked the *Phœnicopter* in the class of aquatic *fissipedes*; but that it is a water-fowl all agree: *Aristophanes* calls it *αἰνῶτις*. i. e. *palustris*; and *Aldrovandus* says of it, *avis est aquas amans*: Not to mention other authors.

Authors are silent as to the different kinds of this bird, only *Aldrovandus* gives us two figures thereof that are not alike.

This bird is found in *Africa*, *America* and *Europe*: *Heliodorus Æthiop. lib. 6.* calls it *Νεῖλωσι γοῖνικονλέπων*, a bird of the Nile; and the old scholiast upon *Juvenal Sat. II. v. 139.* affirms, that *abundans est in Africa*; and *Du Hamel* speaks of it to this purpose, 'amongst animals remarkable for their large size, is that bird from *Egypt*, which the ancients, on account of the red feathers of its wings, called *Phœnicopterus*.' *John De Laet* writes, that they are numerous in the island of *Cuba*, as also at the island, called *Rocca*, lying on the coast of the province of *Venezuela* in *South America*; and *Rockfort* says the same thing of the island of *St. Domingo*: *Dampier New Voyage round the World p. 67.* saw some few of them at *Sal*, one of the *Cape Verde Islands*; he likewise saw some of them at *Rio la Hacha*; as also at an island near the continent of *America* right against *Querisao*, called by the privateers *Flamingo-key*, from the vast number of them that breed there; and he never saw of their nests and young but there only.

Tho' these birds live for the most part in those hot countries, yet they sometimes visit us here in *Europe*, and so may be accounted amongst the migratory kind, or birds of passage, which is confirmed by the testimonies of several authors; for, *Bellonius* tells us, *migrant ultra mare*, and are often taken in *Italy*, and oftner in *Spain*: *Gassendus Vita Peiresc. lib. 2. in fine* says, they are frequently caught in the fenny grounds and marshes about *Arles* in *Provence*, upon the *Rhone*: *Gesner* says, 'a certain person told me that this bird is caught not far from *Monte Passulano*;' and in another place, that they swim in flocks not far from the *French shore* in the *Mediterranean*: *Willoughby* writes, that in hard weather in winter, it comes over to the coast of *Provence* (and is often taken about *Martiquez*, a sea-port town in that country) and in *Languedoc*, and that it is frequently found about *Montpelier*; but from whence it comes, and where it is bred, that, says he, I cannot tell.

N. B.

N. B. this passage is not in the *Latin* edition of his works, but added to the *English* edition, published two years after: However, he positively says, that they do not come from *Flanders*, where they are so far from being common, as some alledge, that none of them was ever seen in that country. Dr. *Charleton de different. & nom. Animal.* says, that he had a present made him of the skin of one of these birds, well stuffed and dried: Dr. *Lister Annot. in Apicium Celium lib. 6. cap. 7.* says, 'the *Phœnicopterus* is common in the marshes on the sea-coasts of *Provence* and *Languedoc*.'

Whether this bird were known to *Aristotle* is a question; for, all our writers of natural history agree, that the *Phœnicopterus* is no where mentioned by name by that philosopher; yet they can hardly believe, that he was ignorant of a bird so plainly described by his cotemporary *Aristophanes*: 'It is surprising, says *Gesner*, that the name of so beautiful a bird is not mentioned by *Aristotle*, since his cotemporary *Aristophanes* speaks of it: But I suppose, that this was a rare bird among the *Greeks*.'

Bellonius thinks, that *Aristotle* described this bird under the name of *Glottis* or *Lingulaca*, as *Theodore Gaza* translates it: *Aldrovandus* is of the same opinion, but *Gesner* and *Scaliger* are not; for, the former says, 'I rank the *Glottis* among these birds, our countrymen call *Gallinule aquarice*, which are all of the *fissipede* kind;' and the latter in his commentary on this passage says, 'I cannot tell what sort of bird the *Glottis* is; what a certain person has said about the *Phœnicopterus* is ridiculous.'

Gesner says, *circa lacus & paludes victitat*, and that it feeds on periwinkles and fish; and by *Dampier's* account we learn, that they love to keep together in flocks, and to feed in mud and ponds, or in such places where there is not much water; that they are very shy, and therefore, that it is hard to shoot them; that they build their nests in shallow ponds, where there is a deal of mud, which they scrape together, making little hillocks like small islands, appearing out of the water, a foot and a half from the bottom; they make the foundation of these hillocks broad, bringing them up tapering to the top, where they leave a small hollow pit to lay their eggs in; and when they either lay their eggs or hatch them, they stand all the while, not on the hillock, but over it, with their legs on the ground in the water, resting themselves against the hillock, and covering the hollow nest upon it with their wings; for, their

their legs are very long; and building thus, as they do, upon the ground, they could neither draw their legs conveniently into their nests, nor sit down upon them otherwise than by resting their whole bodies, to the prejudice of their eggs or young, were it not for this admirable contrivance they have by instinct; they never lay more than three eggs and seldom fewer; the young ones cannot fly till they are almost full grown, but run exceeding fast; thus far *Dampier*.

Du Tertre in his *Histoire des Isles, &c.* gives these farther circumstances; 'these birds, says he, have such a loud cry, that any one would be apt to take it for the sound of a trumpet; they are always in flocks, and whilst they have their heads down muddling in the water, like swans in quest of their food, there is always one of them standing with extended neck and watchful eyes, and with his head always in motion; and as soon as he perceives any one, he sounds the trumpet, and gives the alarm; he is the first to take wing himself, and all the rest follow him; they fly in regular order like cranes; but if it is possible to surprise them, they are so easily killed, that the least wound confines them to the spot; they are pretty rare, and never seen but in the most remote salt marshes: They flea them, and make furs of their skins, which are said to be of service in coldness and weakness of stomach.

Rockfort likewise informs us in his *Histoire des Antilles*; that they are so quick of hearing and smelling, as at a distance to be apprised of hunters and fire-arms; and to prevent all surprise, they chuse open places, and the middle of marshes, from thence at a distance to descry the enemy; and there is always one upon the watch; they are fat and their flesh is delicate enough; they preserve their skins which are cover'd with a soft down, and put them to the same uses as those of swans and vulturs.

De Laet observes, that these birds are so accustomed to salt water, that the *Indians*, when they tame them, mix salt with the fresh water, else they pine and die away; and tho' *Aristophanes* says it is *ἡ τῶν ἰνδαδῶν ἰ. e. not used to be tame*; yet *Gassendus* writes, that *M. Varius* president of the parliament of *Aix* in *Provence*, us'd to divert himself with feeding them with bread moistened with water, which they commonly eat in the night and not in the day time: The same learned person observ'd, that they could discern the approach of cold weather, and come to the fire, so as sometimes to burn their feet;

feet; and that when one foot pained them, they would go upon the other, using their bill instead of the burnt foot; that they slept standing upright on one leg, with the other drawn up to their breast; and lastly, that very little sleep serv'd their turn.

This beautiful and rare bird was much esteemed by the Romans, and chiefly made use of in their costly sacrifices and sumptuous entertainments; thus *Suetonius in vita Caligul.* §. 22. describing the exquisite sacrifices, which were appointed by *Caligula* to be offered to himself as a divinity says; *Hostiæ erant phœnicopteri, pavones, tetraones, numidicæ, meliagri-des, phasianæ, quæ generatim per singulos dies immolarentur*; and the same historian relates farther *Scalig.* §. 57. that this Emperor sacrificing the day before he died, was sprinkled with 'the blood of a *Phœnicopter*'.

That the tongue of this bird was much commended and in great esteem for its excellent taste and most delicious relish, will appear from the following quotations; and first we read in *Pliny Nat. Hist. lib. 10. cap. 48* that *Apicius* said, 'the tongue of a *phœnicopter* was a very delicious and savoury bit'; *Martial* says the same thing in the above cited epigram.

Dat mihi penna rubens nomen; sed lingua gulosis nostra sapit.

And *Juvenal, Sat. 11.* where he exposes the extravagant luxury and gluttony of the Romans, mentions this fowl, amongst some others equally rare, made use of in their feasts,

Et Scythicæ Volucres et Phœnicopterus ingens.

We read in *Sueton. Vitell.* §. 3. how the Emperor *Vitellius* had them often serv'd up at his table, with a great many more varieties brought from the most remote parts of the world, his words are; *In hac scarorum jecinera, Phasianorum cerebella, linguas phœnicopterum, murænarum lactes a Carpathio usque fetoque Hispaniæ per Navarchos ac triremes petitarum commiscuit*; that is, from the remotest parts of the empire, both towards the east and towards the west: And *Heliogabulus* another of the Roman emperors, as *Lampridius* writes, treated his courtiers with sumptuous nice dishes, made of the intrails and brains of *phœnicopters*.

What

What is related by *Gassendus* in the life of *Peureskii* is no argument against the excellent relish of the tongue of this bird; for, his friend *Varius*, who therein seems to contradict the receiv'd opinion, was at that time just upon the recovery from a long illness; he had no appetite; loath'd all sorts of meats, and mended but very slowly; so that it is no wonder, if he did not perceive all the relish of that nice bit, for which of old it was so much commended; besides, his answer seems not to regard the tongue, which was acknowledged to be much sweeter than that of a kid, but the flesh of this bird; as appears from the original.

The method of dressing the *phœnicopter* and making a sauce for it, may be seen in *Apicius's* book *de obsoniis et condimentis, seu de arte coquinariâ lib. 6. c. 7. Phœnicopterum elixas, levas, ornas; includis in cacabum, adjicies aquam, salem & aceti modicum; dimidiâ cocturâ alligas fasciculum porri & coriandri, ut coquatur; prope cocturam, defrutum mittis, coloras; adjicies in mortarium piper, cuminum, coriandrum, laseris radicem, mentham, rutam; fricabis, suffundes acetum, adjicies caryotam; jus de suo sibi perfundes, re-exinanes in eundem cacabum, amilo obligas, jus perfundes & inferes: Aliter, Assas avem, teres piper, ligusticum, apij semen, sesamum, defrutum, petroselinum, mentham, cepam siccâ, caryotam; melle, vino, liquamine, aceto, oleo & defruto temperabis.*

Philostratus reckons the scarlet bird, i. e. the *phœnicopter* among the dainties at feasts *lib. 8. Vita Apoll.* According to *Wormius* in his *Museum*, and *Dr. Grew*, the tongue of this bird, as *Apicius* said was a delicious morsel among the Romans.

N. B. In the treatise *de Obsoniis & Condimentis*, that goes under the name of *Apicius*, there is no mention made of the tongue of this fowl; for, as *Dr. Lister* well observes ' *Apicius* ' says nothing of the exquisite relish of the tongue'; which is a pretty convincing proof, that this book *de re coquinariâ* is only a collection made by some modern Roman; the name of the old *Apicius*, that great master in the art of eating, being only perfix'd to it for the benefit of the bookseller.

According to *Dampier*, the flesh of both young and old is lean and black, yet very good eating, tasting neither fishy nor unsavory, a dish of the tongues of *Flamingo's* being fit for a prince's table; they are large, with a large knob of fat at the root, which is an excellent bit: And according to *Du Tertre*

the flesh of the *Phenicopter* is very good, tho' it taste a little fishy; but especially, the tongue is reckoned the most delicious morsel that can be eaten'.

According to *Bellonius*, this bird is of the bigness of the fowl he calls *Elorius*, which is our *Curlew*: *Scaliger* compares it to the *Heron* in bigness: *Gesner* says it is as big as a *Ciconia* or stork, or rather bigger: *Aldrovandus* writes thus, 'I can determine nothing with certainty about its bigness, because I never saw it': According to *Dampier*, the *Flamingo* is a sort of large fowl much like the *Heron* in shape, but larger and of a reddish colour: According to *Du Tertre*, 'the *Flamingo* is as big as a wild goose'.

According to Mr. *Willoughby* it has an extraordinary long neck: According to *Du Tertre*, 'it has a red neck, and very slender, considering the bigness of the bird, and a *demi-toise* long'. According to *Scaliger*, 'it has a very short tail, or cut short, as it were'.

Scaliger writes, 'that the bill of this fowl is neither streight nor altogether crooked, rather resembling a part of a *Scythian* bow': *Gesner* who compares this bird with a crane for bigness, adds, 'that its beak is almost 1 and $\frac{1}{4}$ as long as that of a crane, being thick on its upper part, and uneven by reason of some tubercles'.

Aldrovandus approves of the account *Scaliger* gives of the bill, and then adds, 'there is no small *lusus naturæ* in the formation of the bill; for, it is not flat like that of ducks and geese, tho' otherwise broad; nor is it streight and round like that of herons; nor lastly, is it crooked like that of eagles or hawks; tho' it be true, that it is recurvated and bent downwards, but it has a remarkable protuberance in the middle of the upper bill; it is six inches in length, and internally hollow, and channell'd in the middle; the upper bill is longer than the lower, and terminates in a very sharp point: but on the contrary, the lower is a great deal thicker': According to *Du Tertre*, 'it has a round small head, with a large beak, four inches long, half red and half black, and bent like a spoon'.

Olaus Wormius gives the following description of the *Phenicopter*; 'the whole head including the bill, was upwards of eight inches long, but without the bill three inches; the bill itself is of the figure delineated by *Scaliger*, it is pretty thick in the middle, but hollow, and channell'd on its upper part on each side; at its origin it has two large holes design'd for

'for smelling; it is bent at its extremity, and internally indented with an eminence in the middle; the inferior part is black, with several *striae*, running out to its extremity; it is not so long as the upper bill, but large and capacious enough, to contain its thick tongue'.

Dr. Grew gives a very curious account of the bill of this bird, for which he says it is most remarkable; the figure of each beak is a true hyperbola; the upper jaw is ridged behind, plain or flat before, and pointed like a sword, with the extremity bent a little downwards; within it has an angle or sharp ridge, which runs all along the middle, at the top of the hyperbola, not above a quarter of an inch high, and hollow, and the margins strangely expanded inwards, for the breadth of above a quarter of an inch, and somewhat convexly; they are both furnish'd with black teeth, as he calls them, from their use, of an unusual figure, *viz.* slender, numerous and parallel as in ivory combs; but also very short, scarce the eighth part of an inch deep; an admirable mechanism, by means of which and the sharp ridge above mentioned, this bird holds his slippery prey the more firmly.

'Mr. Ray describes the beak as somewhat broad, and of a peculiar shape; the upper-bill incurvated, depress'd and indented; the interior thicker than the other.' *Menippus* the *Cynic* philosopher in a fragment of his *de Homine*, as quoted by *Rondeletius lib. de Amphib. c. 5.* affirms, that this bird moves his upper jaw, his words are to the following purpose: 'But that is not peculiar to the crocodile alone; for, among birds, the *Phœnicopter* moves the upper beak, as *Menippus* the *Cynic* philosopher has observ'd in his book *de Homine*.'

Gesner makes the very same quotation from *Rondeletius: Cardan de Varietate rerum lib. 7. c. 37.* repeats the same thing without mentioning any author, to this purpose; 'for, some think that even *Phœnicopters* move the upper mandible,' and subjoins, 'but the reason is not so manifest in a bird, as in a crocodile.' *Wormius* is of the same opinion, but with *Cardan* thinks the cause not so manifest as in the crocodile: Dr. *Charleston* says, that *Cardan* was the first who made that observation: Dr. *Grew* argues for this motion from the peculiar structure of the beak, alledging, however, that there can be no determination of these matters, without inspecting the muscles, and the articulation of the bones; as for the *Phœnicopter*, says he, it must needs be allow'd, that the shape and bigness of the upper beak (which here, contrary to

what it is in all other birds he had seen, is thinner and much smaller than the lower) speak it to be more fit for motion, or to make the appulse, and be received by the lower.

Bellonius remarks, that the legs of this bird are very long; and on the contrary, *Scaliger* writes, 'its legs and feet are so short, that as *Galen* has allowed the longest legs to a man, so the shortest to this bird;' and for this, he is tax'd by *Dr. Charleton*, and *Dr. Grew*; when *Scaliger*, therefore, says, that this bird has the shortest legs of any animal yet known, he would have said the longest: *Gesner* says, 'it has red legs, as long as those of a stork, or longer': According to *Du Tertre*, 'it is the tallest bird I ever saw; its legs are altogether red, and its feet half sea-green:' According to *Rockfort* in his *Hist. des Antilles, Edit. Rotterd. p. 583*, 'its legs and thighs are so high, that the rest of its body is rais'd from the ground two feet or thereabouts.'

Scaliger thus elegantly expresses the fine colour of its wings, *cinereum colorem nobilitans alarum puniceæ pennæ.*

Aldrovandus says; 'it is surprising that *Scaliger* did not take notice of the black colour of its wings; all the rest of the body is a scarlet, white and ash-colour mix'd together:' *Gesner* says, 'the feathers on the upper parts of its body are white, and those on its neck, breast, belly and wings are red;' and speaking of one taken near *Montpellier*, he says, 'its whole body is white, excepting those parts in the wings that are black in storks.' *De Laet* in his *Hist. de Nouv. Monde, lib. 1. c. 9 & lib. 18. c. 15.* observes, 'that they have different colours; for, while they are young, their feathers are white; but as they grow up they become of a rose-colour, and in fine, when they are old, they are altogether of a carnation-colour; there are found some of the same birds near *Montpellier*, that have only the under parts of their wings and body, of a carnation, and the upper parts of a black colour; there are some likewise seen in the isles that have some white and some black feathers intermix'd in their wings:' *Du Tertre* gives much the same account, 'the young ones are much whiter than the old, and they grow red according as they advance in years; I have likewise seen some that had red, black and white intermix'd in their wings, and I suppose these to be the males:' *Constantinus* in his *Lexicon Græco-Lat.* says, 'that their beak, legs and part of their wings glitter with a scarlet colour:' *Willoughby* says, that the neck and body are white; the *alarum remiges* or quill-feathers of the wings black; but

but the *vestitrices* or covert-feathers entirely dyed with a very beautiful, bright purple, or flame-colour, whence it has its name: According to *Dampier*, the young ones at first are of a light grey, and as their wing-feathers spring out, they grow darker, and never come to their true colour, or any beautiful shape, under 10 or 11 months old; when many of them are standing together by a pond-side, at half a mile's distance, they appear like a brick wall, their feathers being of the colour of a new red brick; and they commonly stand upright and single, one by one exactly in a row, except when feeding, and close by each other.

All authors agree in the red colour of its legs and feet; thus *Scaliger*, 'its legs and feet are of the same colour with its wings.'

'As to the colour of its beak, *Gesner* says, it is as red as blood': *Aldrovandus* writes, 'that part which regards the forehead inclines from a white to a chesnut-colour, and all the rest is black'; *Willoughby* affirms that the tip of the bill is black, or of a dark blue.

The whole fowl is delineated by *Gesner* and *Aldrovandus*; and *Dr. Grew* has given us the figure of the head and bill, as he found it among the rarities in *Gresham-College*.

N. B. The figure of the *Phœnicopter* in *Willoughby* is copied from the second figure of *Aldrovandus*: *Gesner* says, that the *Phœnicopter* from which his figure was drawn, was sent him by *Rondeletius*: *Aldrovandus* had the first of his figures from *Sardignia*; and the second which he calls *Phœnicopter alter rostro lato*, was given him by that famous botanist *Carolus Clusius*, he acknowledges, that he never saw the bird itself: In *De Rochfort*, the body and neck of the *Flammant* is pretty well delineated; but the legs, bill, and claws are not.

Du Hamel in his *Hist. Acad. Roy. Edit. Paris*, p. 213. gives a very exact account of this bird to the following purpose, 'it has a pretty long neck, long legs, and a small but firm foot; its eyes are small and ruddy; the *cystis fellea* hangs down from the lower part of the liver; the vessel, by which it hangs, and receives the bile, is large, contrary to what is observed in men and quadrupeds; for, in these the roots of the gall-bladder are exceeding slender; the *oesophagus* is very narrow at its origin, and becoming gradually broader, it terminates in a craw or larger bag; its gizzard is almost the same with a hen's, tho' it does not feed upon grain, but small shell-fish, which are mace-

macerated like grain by the muscles of the gizzard; its wings, when spread abroad, exhibit that flame-like colour, whence it is commonly called *Flamand*, not because it is found in the *Netherlands*, but because its feathers, seen thro' a transparent membrane, exhibit a flame-colour; there is scarce any fowl larger; both its upper and lower beak is incurvated downwards, which is very singular in that fowl; for, it is bent like a plough, whence it is commonly called *Becharii, quasi Aratri-rostrum*. *Du Hamel* tells us that the *Phanicoptes* was dissected at *Paris* by *M. Perrault*.

The following figures were drawn from a *Flamingo*, sent *Mr. Botley* to be stuffed.

Fig. 22. Plate VIII. represents a side-view of the head and bill.

Fig. 23. Represents a front view of the same parts.

Fig. 24. Represents the underside of the tongue next the under-bill; *a*, a cartilaginous substance covering the tip or extremity of the tongue; *b*, a glandulous substance at its basis; *c*, the horns of the *Os hyoides*.

Fig. 25. Represents the upper side of the tongue, upon which are to be seen two rows of strong nervous *papillæ*; their apices or points turning inwards, for the better retention of its prey.

Fig. 26. Represents a side-view of the tongue, to shew the true figure of these *papillæ*, which, being hook'd and turned backwards, prevent, in a great measure, the return of any little animal swallow'd alive.

Fig. 27. Represents the *Cornua, Os hyodæum*, as big as the life, and as all the other parts are.

The Occultation of a fix'd Star in Gemini by Jupiter, Jan. 11. 1717. O. S. and a transit of Mars below the Northern Star in the Forehead of Scorpio, the 5. of Feb. following, in the Morning. Phil. Trans. N° 351. p. 546. Translated from the Latin.

IN Phil. Trans. N° 344. p. 294. we hinted to the curious, that there was to be an occultation of a fixed star by *Jupiter*, and desired they would observe so very uncommon a phenomenon, and of such considerable use in Astronomy, signifying at the same time, that this would happen the 10th of Jan. 1717. But *Jupiter* being almost stationary, and advanc'd towards the east a little farther than by our tables, the said occultation did not happen before the 11th; and was not observ'd at *London*, as could be wished, by reason of clouds.

Yet Mr. *Foulkes* with some others of the *Royal Society* saw at *London* on the 11. of *January* 8^h P. M. *Jupiter's* center at the distance of one diameter of its body, follow the fix'd star, which was more northerly than the said center about three quarters of *Jupiter's* semidiameter; afterwards *Jupiter* was covered with clouds; but from his motion, Mr. *Foulkes* concluded, that the star was in conjunction with *Jupiter* a little after midnight, and hid by the northern part of his disk.

At *Westminster* Mr. *Desaguliers* and Mr. *Gray* saw the fix'd star at 6 o' clock in the evening, distant an entire diameter of *Jupiter* from his limb towards N. W. whence, and from the observations made the following days, it appears, that the conjunction happened about midnight.

At *Wansted* Mr. *Pound* made the following accurate observations, with a pretty long telescope and a micrometer.

On the 5th of *January* at 5^h 6' equated time, *Jupiter's* center was distant from the said fix'd star 31' 49" which at 5^h 38' it followed 34' 12" of right ascension; and at the same time the southern limb of *Jupiter* had the same declination with the star.

On the 9th of *January* at 6^h 6' *Jupiter's* center was distant from the star 10' 49", and 8 minutes after, the difference of right ascension was 11' 32" at which time the planet's center was but a small matter more southerly than the star.

On the 11th of *January* at 5^h 30' equated time, the distance of their centers was 1', 24"; and at the same time the star was seen more northerly than *Jupiter's* center about one fourth of his diameter; and *Jupiter's* least diameter was found to be 43"; then clouds came on.

On the 12th of *January* at 5^h 17' the distance of the centers was 3' 7" and at 5^h 30' *Jupiter* preceeded the star 3' 30" of right ascension; and at the same time the northern limb of *Jupiter* had the same declination accurately with the fixed star.

It is manifest from comparing these observations, that this fixed star was in conjunction with *Jupiter* on the 11. of *January* at about 13^h, and only 17" or 18" more northerly than his center, and consequently hid by him.

This fix'd star, tho' hitherto in no catalogue, was then in 22° 13' of *Gemini*, with 13' and $\frac{1}{2}$ S. Lat. and had a star accompanying it, that preceeded it 17', and was 7' more northerly, or in 21° 56' of *Gemini* with 6' and $\frac{1}{2}$ S. Lat. with which *Jupiter* seemed to be in conjunction on the 16th of *January* at 6^h 30' in the evening: Thus in less than two months time, *Jupiter* eclips'd with his body two fix'd stars, of which there

is not, one single instance extant 'since the invention of telescopes; consequently, such observations are of great value and worthy to be transmitted to posterity: According to *Gassendus's* observation, our little star was in conjunction with *Jupiter*, when stationary, *Feb. 6. 1634.* and more southerly by three of his diameters; whence it will appear, upon a just calculation, that *Jupiter's* nodes have not sensibly mov'd for these 83 years last past and that in $2^{\circ} 8' 35''$ from the first of *Aries*.

The same Astronomers have been as diligent in making that other observation, no less remarkable, of *Mars's* transit near the north star in the forehead of *Scorpio*; for, on the fifth of *February* in the morning, or the fourth, 16^h *Mars* was seen so near the said star, that it could not be discerned by the naked eye; but by the telescope it was found above it, and to the east, consequently, *Mars* was not yet in conjunction with it; at $16^h 10'$ apparent time, *Mars* was in a streight line with the northern star of *Scorpio's* forehead, and the telescopic star, which follows it, to the north, at about $8'$ distance: At $16^h 35'$ *Mars* was in a right line between the north, and middle star of the forehead, and a quarter of an hour after, between the south star of the forehead; so that the conjunction was estimated as to Long. at $16^h 54'$ apparent time; at which time *Mars* was only two minutes pretty accurately more southerly than the star. Mr. *Pound* also observ'd the conjunction as to right ascension, at $17^h 25'$ apparent time, with the distance of the centers $2' 7''$: It was an agreeable sight to see *Mars* gradually entering into the star, and plainly discovering his motion, tho' otherwise exceeding slow: Compare this with *Horrox's* observation *Feb. 7.* in the morning, 1638, which see in his *Letters* p. 304; for, at that time *Mars* coming to the same star, approached it much nearer, but the conjunction was over before his rising: Add to these an observation of *Saturn* made by Mr. *Pound* on the 25th of *January* $12^h 25'$ equated time, when the planet was distant from the 58th star of *Virgo* in *Catal. Britan.* $13' 16''$ towards the south, and the star in $19^{\circ} 21' 52''$ of *Libra*, with $2^{\circ} 47' 25''$ N. Lat. followed it $2' 30''$ right ascension.

An Account of a tessellated Pavement, a Bath and other Roman Antiquities, discover'd near East Bourne in Suffex; by Dr. Tabor. Phil. Trans. N^o 351. p. 549.

THE meadow, in which the greatest part of this tessellated pavement lies, is almost a mile and a half south east of Bourne; it contains about four acres, and is of a triangular form, the south side is towards the sea, on the north side of the meadow is a highway, leading from Bourne to Pevensey; the west side is separated from a large corn-field, by a fence of posts and rails; about the middle of this fence is the pavement, distant from high water-mark a furlong; in former times it might have been somewhat more, because from this point to the westward, the sea is always gaining on the land. This pavement was little more than a foot below the common surface of the ground; what lay next it was a small sea-gravel; its position is nearly due east and west, its length is 17 feet four inches; its breadth 11 feet; at first it seemed to have been bounded with a thin brick set on edge, about an inch above the *tesserae*, so exactly straight and even, as if shot with a plane, and so well cemented, as if it had been one entire brick: But when the outside of the pavement was broken up, instead of bricks set on edge, as was imagined, it was bounded with a border of bricks laid flat, with their ends next the *tesserae* turned up; the thickness of these bricks was an inch and a quarter; their breadth between 11 and 12 inches; their length was full 13 inches, and before they were turned up on their ends, could not have been less than 17 inches; they were very firm, and not in the least warp'd or cast in the burning; when broken, their substance was fine and well mix'd, of a uniform and clean a red colour, as a piece of fine bole, excepting at the ends where turn'd up, being all over cover'd with a plaister (the same which *Vitruvius* calls the *Nucleus*) half an inch thick; so hard, entire and even, that it seemed one entire stone, quite round the pavement: Next within the bricks, there was a list or border of white *tesserae*, 13 inches broad; within that a list of brown *tesserae* (somewhat darker than the whet-stone, and somewhat lighter coloured than the touch-stone) four inches broad; then a list of the white, five inches broad; next within that, another list of the brown, four inches broad; all the rest of the pavement was set with white *tesserae*, without any ornament or figure.

When the ground about the pavement was dug up, on the north side was discovered an entire bath 16 feet long, five feet nine inches broad, and two feet nine inches deep: It was fill'd with rubbish of buildings, which seemed to have been burnt, to wit, hard mortar adhering to pieces of *Roman* brick, squar'd stones, and headed flint, mix'd with ashes and coals of wood; from the north-west corner of the pavement was the passage into the bath, three feet three inches wide, at which place the bricks, that bounded the pavement, were not turned up at their ends, but lay even with the *tesserae*; at the distance of 15 inches from the *tesserae* there was a fall of two inches to the landing place out of the bath; the landing place was also three feet three inches long, and two feet two inches broad; from thence the descent into the bath was by two steps of stairs, the length of the stairs being the same as that of the landing place; the breadth of each step was 11 inches; the height of each step a little more than ten inches; the lowest step was 20 inches from the farther side of the bath; the whole work was very compact, and exactly well made, and not in the least injur'd by time, nor by the violence it underwent when fill'd up; justly answering to *Vitruvius's* precepts de *Architec. lib. 2. c. 3.*

The pavement was secur'd on every side, and its edges rested on a very firm and neat built wall, made of *Roman* brick, squar'd stone and headed flint, between five and six feet deep below the surface of the pavement, and full 23 inches thick, which we may suppose to be two feet *Roman* measure; the bricks were not in regular courses, as in *Roman* buildings above ground, but without any order disposed of in the wall; the top of the wall, it is true, was but 15 inches thick, and cover'd with the bricks, which bounded the pavement; but about 14 inches below the top, there was a set-off on the inside of the wall, eight inches broad; when the foundation was bor'd quite thro', there was observed next under the *tesserae* a bed of very strong mortar, upwards of a foot thick; and under the mortar a bed of clay two foot thick, and under that a firm foundation of brick; the clay (which the ground thereabouts does not afford) was very fine, red, and close, having been, no doubt, carefully ramm'd down; the surface of the clay was neatly pitch'd with small flint and pebbles, pointed at their lower end and headed at their upper ends; this pitching or paving is by *Vitruvius* call'd *statuminatio*, and the stones, it is done with, *statumina*; he directs them to be set, when the under work is made sound and firm by well ramming it down.

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This pitch'd work was exactly even with the set-off in the inside of the wall; upon it was laid a bed of coarse mortar of about nine inches thick; the skirts of this mortar (which *Vitruvius* calls *rudus*) rested on the set-off above-mentioned: it was compos'd of lime, a sharp coarse sand, small pebbles and bits of bricks; upon this *rudus* was a finer composition, made as near as Dr. Tabor could guess, with lime, a fine sharp sand, some kind of ashes, and (which was the greater part) stamp'd bricks and potsherds, in grains not exceeding cabbage-seed, the flower or fine powder being separated therefrom: This bed was about half a foot thick, and is what *Vitruvius* calls the *nucleus*; whether we may call it *terrace*, the Dr. leaves others to determine: Both this *nucleus* and *rudus* under it nearly equalled *Pentland-stone* in hardness and compactness; on this *nucleus* or *terrace*, the *tesserae* were set on end; but with such exactness, that two sorts of cement were used to fix them; their lower ends stood in a cement of lime only, well work'd; their upper halves were cemented with a fine grey mortar, consisting of fine sand (and as it seem'd) ashes and lime; this grey cement every where fill'd the intervals at their heads, and was much harder than the *tesserae* themselves.

The *tesserae* were only of two colours; viz. white and a dark brown, and harder than a glaz'd and well burnt tobacco-pipe, and of a somewhat finer grit; the brown seem'd to be of the same substance with the white, but colour'd by art, as *Pliny Secun. Hist. Mund. lib. 35. c. 12*, informs us the workers in clay of old had a method of doing, they seem'd to have been formed in a mould, and afterwards burnt; hence the Dr. takes *Vitruvius's* meaning where he makes so plain a distinction between the *tesserae*, and the *scutellia*, that the one was, according to the import of the name, formed by instruments out of stone, brick and tyle; the other shap'd in a mould and burnt; they were not of an equal size, none exceeding an inch in length; the shortest was $\frac{4}{10}$ of an inch; most of them were equally formed their whole length; but of some the lower ends terminated almost as sharp as a wedge, on purpose, as may be suppos'd, to be driven where any interstices were left; and also at their heads they were not all equal alike, some being exactly square, some oblong, some semi-lunar, but none triangular; the diameter of those that were square, was about $\frac{4}{10}$ of an inch; the longest side of those that were oblong at the head was little above $\frac{4}{10}$ an inch: The preparations for fixing this pavement exceed those *Vitruvius* prescribes in the firm wall near six feet

below the surface, in the bed of clay within it two foot thick, and in the foundation of brick under the clay; but when we consider, that the situation of the ground here is low, and not many feet higher than the sea at spring tides; and that it might as well be annoy'd by land floods after great rains, as by water ouzing thro' the earth from the sea so near it, whence the work might in time receive damage, we must allow the above-mentioned additions to be the result of a very judicious foresight.

The bath was also formed and secured by a very compact wall, of the same breadth and depth with that on which the pavement rested; the wall, which sustained the north side of the pavement, formed the south side of the bath, where from the east end to the end of the stairs there was a solid seat, 12 foot 9 inches long, nearly 10 inches broad, and 14 inches high; the bottom or floor of the bath was made after the same manner as the pavement, excepting the *ressera* and the thick bed of clay; for, under all there was brick, then a bed of the *rudus* or coarse mortar somewhat more than a foot thick, over that the *nucleus* or terrace only half a foot thick; the sides of the bath, the seat and the stairs were plaistered over with this terrace about half an inch thick; all which were throughout so hard, compact and smooth, that when first opened, the whole seemed, as if it had been hewn out of one entire rock, and polished; at the middle of the east end at the bottom, there was a sink-hole a little more than three inches long, and upwards of two inches deep; about four inches above it, there was another passage thro' the wall of the same size; the first we may suppose to let out the water which had been used, and the other to let in fresh water; the stairs and seat were chiefly made of *Roman* brick, between 15 and 17 inches long, between 11 and 12 broad, and near one and a half thick: At the north side of the bath the ground was not opened, but at the east end of the bath and pavement; at the south side of the pavement, and at the west end of both, there seemed to have been several vaults or cellars: for, there were very firm walls 23 inches thick continued every way, whose foundations were as low, as those which supported the pavement; so that to the depth of six foot the ground was filled with such rubbish, as was taken out of the bath; the bricks in this rubbish, which were all broken, had several degrees of thickness, from three to a little more than one inch; some had one of their sides undulated, some fretted, and others had roses on them well imitated; there were two sorts of channell'd bricks; the one like a trough, the chan-

and three inches broad, and as many deep, the brick itself being one inch and a half thick; the other sort had a cylindrical channel, so that when two were clapped together, they formed a hollow cylinder of three inches diameter; these channell'd bricks being all broken, their length when whole was uncertain, as also the use they served for, whether for gutters to convey water, or whether they were placed in the walls to distribute heat throughout the building, as was usual in the ancient structures at Rome.

It is farther observable, when the ground was opened the second time, that from the south-west corner of the pavement, five foot lower than the surface of the pavement, there was discovered a large space, paved with brick 11 inches broad, almost one inch and $\frac{1}{2}$ thick, and 15 inches long, having two courses of this brick; there was $\frac{1}{2}$ a foot of mortar under the lower course, and about an inch of mortar between the two courses; these bricks were also perfectly well made, but on the under side of each were two knobs, about the size of half a walnut, fixed on them, as is probable, to keep them steady, till the mortar they were set in should dry; this paved place was searched six or eight foot every way; it was all covered with a coat, about two inches thick, of ashes and large coals of wood; on that lay confusedly large pieces of the *rudus* or coarse mortar abovementioned, and lumps of the *tesserae* in all respects like those on the pavement, and cemented in the same manner; moreover, there were mixed with the ashes several large iron nails, bigger, but not quite so long, as those called double tens; some hooks for doors to swing on; several small pieces of earthen ware, some resembling bits of urns; some of a fine yellow clay; some red, thin, neatly wrought and adorned with flowers; and lastly, part of a human skull, and pieces of bones near it; which bones were not inclosed in any vessel, but lay loose; they were discoloured like those seen in urns; so that the body they belonged to might perish by the same flames, whereby these buildings were destroyed: There was no inscription found either on stone or brick; no statue or other figure, excepting those on the bricks already mentioned; nor were there any coins met with there; but somewhat more than a furlong to the north-west of these works, in digging the foundations for a malt-house there was found a coin of *Posthumus*; and in the ground dug up for the foundation of a dwelling house a coin of *Constantine's*.

From the nearness of the bath, it may reasonably be concluded, that the pavement was neither a part of a temple, nor a court of justice; the continuation of the foundations that might be

be traced every way from it, and what was last discovered, rather shew that it was an apartment of a magnificent palace.

Pliny *sec. Hist. lib. 36. cap. 25*, supposed, that these *lithostrota* or tessellated pavements had their original in Greece; but, perhaps, the Greeks borrowed their patterns from Asia; for, from the book of *Esther* *ch. 1. v. 6*. we learn, there was a most royal banquet at *Susa*, on a *Adespalon* (so the *Septuagint* has it) of costly stones, 400 years before the time of *Sylla*, who brought them first into Italy: *Josephus* against *Appion* book 2. affirms, that the *Grecian* laws, learning and arts were fetched from Asia; and indeed, when we reflect on the antiquity of the *Levitical* law, the pyramids of *Egypt*, the temple of *Solomon*, the walls and palaces of *Babylon*, and the sumptuous remains of *Palmyra* and *Persopolis*, we have no reason to reckon the Greeks, authors, but rather good imitators of those early examples of learning and arts.

When *Quintus Cicero* accompanied *Caesar* the second time he invaded *Britain*; his brother *Tully* had the oversight of some buildings he had appointed to be made in the *Villa Manliana* at *Arzano*, and in a letter *Tully* sent into *Britain*, *Cic. ad Quint. Frat. lib. 3. epist. 1*. informs *Quintus*, that he was well pleased with the feat, and the more so, because the pavemented piazza was magnificent; that the pavement seemed to be exactly well made; that he had directed some chambers to be altered, because he did not approve of them; that he had removed the sweating-room in the bathing apartment, into another corner of the *apodyterium*; and afterwards in the same letter he makes mention of such another work: Again, about the time *Quintus* returned out of *Britain*, and was fixed with the legion he presided over, in winter quarters among the *Nervii* (of whom *Caesar* makes mention in his commentaries) *Tully* *ibid. epist. 3*, takes notice of a pavement that was likewise making for himself. It is hinted by *Varro*, *Ter. de re rust. lib. 3*, that a *Lithostroton* was one of the members of a complete *Villa*: *Varro* was 80 years of age when his books *de re rustica* were composed; *Tully* was something more than 50 when the above-cited epistles were wrote: *Caesar*, in *Suet. Tranq. Jul. Caf. cap. 46*. when a general, made the *tesserae* and *sestilia* for pavements to be part of his baggage; and *Vitruvius* *lib. 7. cap. 1*. contemporary with these three, calls the *lithostrota*, *principia expolitionum*, which makes it evident, that these floors were held in esteem.

From the whole we may observe, that some time before, and in the first age of the empire, the humour for these kinds of floor-

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floorings prevailed much among the Romans; wherefore it is not at all surprising, that they are found in so many parts of this island: But in the time of *Pliny* they began to be out of use on the ground, tho' still made above stairs or in his own words *in cameris* or chambers *Hist. lib. 36. cap. 25.* Whether the *libostro* in chambers were usual in *Vitruvius's* days we have no warrant to suppose from any hint in his writings, notwithstanding he gives rules for making them *plano pede*, i. e. on the ground, and *sub dio* (which from the method prescribed by him must be) *alops*; because for sustaining those latter he orders the work underneath to be well secured with two layers of planks a-cross each other and nailed down; and then the *statuminario* or pitching the mortar, terrace and *terrefra* as before on the ground, *Vitruv. lib. 7. cap. 1.* But because by *sub dio* *Vitruvius* could not mean chambers; and tho' *Pliny Hist. lib. 35. cap. 25.* informs us, that the Greeks used to cover or flat-roof their houses with these pavements; yet since neither *Vitruvius* nor *Pliny* mention any such mode prevailing in their time at Rome; it remains, that we suppose, the *sub dio*, or *Subdialia* of *Vitruvius* to mean pavements, mounted on pillars or arches, which might afford delightful terraces out of the upper rooms, and shady piazzas underneath; and in this sense, perhaps, may be understood *Tully's porticus pavimentata* above-mentioned: By the several apartments, which the foundations of these rocks point out, there seems to have been nothing wherein the buildings that once stood there, might come short of the magnificent structures, with which the Romans gratified their luxury; the uses each were designed for cannot be determined, nor whether there was a piazza covered with a *libostro*; but be that as it will, it is next to demonstration, that there was some upper floor, sustained by wood, and paved with the *terrefra*, after the same manner as *Vitruvius* directs; and on the brick pavement, last discovered, the coat of ashes and wood-coals with nails therein, covered with large pieces of the *rudus* and *terrefra* well cemented together, and the *nucleus* adhering to them, shew, that there was an upper pavement broken by its fall, when the fire had consumed its support.

As to the Roman architecture, it may not be amiss to observe, that when they designed a building, they could not immediately begin it, their preparations requiring a competent time. By their well shaped, durable bricks and by their stone-like mortar, we may plainly perceive, they built not with such hasty materials, as are now used. Both *Vitruvius lib. 2. cap. 3.* and *Pliny Hist. lib.*

lib. 35. cap. 14. direct bricks to be made in the spring, and to be two years a-drying; and where *Pliny* speaks of their mortar, *Hist. lib. 36. cap. 23*, he says, it was ordained by the old laws of *Rome*, that no undertaker should build a house with mortar, which had not been made three years before; we find, indeed, that their walls seem to bid fair for eternity, whereas, ours from parcimony and ill management, are scarce able to endure one age.

An Account of the Nature and Virtues of the Pyrmont Waters, with some Observations on their Chalybeate Quality; by Dr. Stare. Phil. Transl. N^o 351. p. 564.

DR. *Stare*, having procured about a dozen quarts of *Pyrmont* waters, made some experiments with them; by the taste he found, that they contained a rich chalybeate virtue and also that they made a very brisk and lively impression on the palate, more grateful and spirituous than the best *Spaw* waters he ever tasted: The *Spaw* waters are looked upon to be extraordinary good, if they sparkle a little in the glass; but these *Pyrmont* waters in summer time, when poured into the glass, nay sometimes even in the bottle, as soon as the cork is drawn and the air admitted, make a remarkable ebullition, somewhat like bottled cyder, tho' this was soon over; but they still retained their smart and brisk taste, and high chalybeate relish to the last drop, tho' some hours were spent in drinking them off: In the winter time these waters neither sparkle, nor ferment, at least what the Dr. had did not; but this might be owing to their not being carefully kept, being exposed in cold cellars, where his beer and wine stood in the winter, and yet they did not lose their chalybeate taste, nor their pleasant brisk goût: These waters have been reckoned in the number of the *German Saur Brunnen* or *acidules*; and some, to whom the Dr. gave a glass of the water, ascribed a sharp taste thereto, and were apt to have an opinion of its being sour; but upon considering better, whether that taste were acid or really sour; they acknowledged, that the smart and brisk taste misled them to call it sour: Thus cyder and soft ale, when bottled, will cause such a pungency on the palate, when it is far from being sour; and even volatile *alkalies* of *sal armoniac* or of hartshorn may be made to give the like pungency to the tongue,

In order to a more nice enquiry into the acidity of these *Pyrmont* waters, the Dr. dropped into them considerable quantities both of spirit of hartshorn and of spirit of *sal armoniac*, and both

both justly prepared; but could not discover the least Inflation or motion upon this conjunction as usually with an acid.

He made still more nice and certain experiments of these waters, by mixing milk with them, sometimes in equal, and sometimes in double the proportion, and in various degrees of warmth, both lukewarm and boiling hot, but he could perceive no curdling; but rather on the contrary the water preserved the milk from coagulation for four or five days, even in *September*, when it was hot weather.

Take about half a grain of powdered galls to a glass of a quarter of a pint, and this in a moment renders it turbid and makes a dark purple, especially if you stir it; but if you drop the powder on the surface of the same water, it then communicates a fine blue tincture: In order to give a very fine tincture, take five leaves of strong green tea, put them into the bottom of a glass that holds a quarter of a pint, and the leaves will unfold themselves, and in a quarter of an hour tinge the water with such a beautiful azure blue, as few vegetables afford the like; observe, that the longer these leaves, or any other stiptics (which are the *precipitators*) stay together, the more they degenerate into a deep purple, or even to an atramentarious colour.

As to the internal use of these waters the Dr. drank about a quart at a time, in the following manner; he first began with the *Sparw* waters, which he procured very good, and drank them for a week, and they agreed with him very well; he then drank the *Pymont* waters for three or four days, and continued the use of both these waters alternately, till he had drank of them for about 20 days: By the result of his experiment, it seemed very plain to the Dr. that the *Pymont* water was more agreeable, gave more strength and spirit, and was as much or more preferable for its internal virtue, as for its excelling the other in a brisker and sprightlier taste.

There is another excellency in these waters, which will render them more useful to us than any foreign chalybeate waters hitherto known; for they will keep better, and they are not so soon spoiled by any accidental insinuations of air, as the *Sparw* waters are apt to be; the chalybeate mineral is here thoroughly dissolved, well united and mixed in this water, so that it does not easily precipitate; for which reason it may also the better pass the *vasa lactea*, and even enter into the mass of blood itself, and so produce the more considerable effects; that this is not a bare hypothesis may be proved by the following experiment.

Having exposed some *Spaw* water in a bottle that was half full and unstopped for 12 hours, he examined it and found it taste just like common water; but the *Pyrmont* waters that were exposed to the air after the same manner, tasted strong of the mineral, and imparted their tincture as at first; nay they continued thus for full two days, and perhaps might have done so longer.

The Dr. thinks he may fairly conclude, that since the *Spaw* has been found very beneficial to our patients in chronical diseases, these *Pyrmont* waters of a much superior virtue will surpass them in conquering many of our obstinate distempers.

In order to discover, whether the *Pyrmont* waters really contain any purging ingredients or qualities, the Dr. evaporated about a quart of this water *ad siccitatem*, he then poured upon the *reliquæ* so much rain water, enough to dissolve the salts, and evaporating that water, he obtained a grain or two of the salts, that tasted muriatic, as most river and pump waters do: It is well known, that the purging waters have a very bitter taste; which the learned Dr. Grew called *sal catharticum amarum*, which distinguished it from all other species of natural salts; that of the *Pyrmont* water has no relation to this, but rather to sea-salt, it not being bitter in the least.

It is also well known, that unless our waters be impregnated with a considerable quantity of this bitter salt, they will not purge at all; two or three grains signify nothing, nor have the least cathartic power; for example, put two drachms of the purging salts to a quart of common water, and this quantity will give but a stool or two to a person who is naturally very easy to work upon: The Dr. examined several other chalybeate waters and found much the like ingredients, and never any that he could suspect to have any purging quality.

The Dr. thinks, it may much better be demonstrated, that the chalybeate waters contain stiptic and astringent qualities, because they owe their original to the iron mineral, and more particularly to the *Pyrites*, which Dr. Lister suggests (not without some reason) to be the parent even of all iron oars, as it is doubtless, the cause of all chalybeate waters: Thus Dr. Stare had often examined the solution of the *Pyrites* by rain-water at *Deptford*, and at other places where copperas is made, and found it a very strong chalybeate water: It is from this mineral we have our strong stiptic and constringent medicines for external and internal uses; from hence we have our powders and salts of steel, or vitriol of *Mars*; nay, even those obstinate and inveterate *Diar-*

rhoeas, which have baffled the force of all medicines, have been cured by a judicious use of *Tunbridge* and other iron waters.

But notwithstanding all that can be said, it will be retorted, that there is matter of fact and experience against this, that the waters do really purge at *Pymont*, where they are drank; it is allowed, that *Tunbridge* waters do not only purge, but sometimes vomit, when drank hastily and in great quantities; but our physicians have corrected this irregularity, and no such complaints are heard of, where they observe a proper *Regimen*; and it is agreed on all hands, that those waters are, in their own nature, binding, and often require some opening medicine; the quantities of water drank at *Pymont* are very large, often two or three *English* quarts; it is not surprising, that their weight forces them thro' the bowels; for, any common water, drank hastily and in such quantity will do the same; whereas, if you take this method and drink *Pymont* or any other chalybeate waters lieisurely, to wit, a pint-glass in an hour, or rather two half pint glasses, you may drink three pints in so many hours without danger of losing them by dejection; and if any one will be careful, and take this caution along with him, he will scarce fail of success, that is, let him be very quiet and still, both in body and mind; the less he stirs or walks, the better he will pass off his waters by urine: He further observes, that none of our *English* steel waters strike such a purple as the foreign do; for, ours communicate a more turbid and dark colour, and the worse the waters are, the blacker sediment they make; those of *Islington* abound with a coarse oker, the mineral is not well dissolved, but gives an atramentarious colour; but the *Pymont* waters excel in their bright azure lustre all the Dr. ever examined.

Most of the above-mentioned experiments were repeated by Dr. *Stare* before the *Royal Society*; and it was found, that the *Pymont* waters communicated a much brighter tincture with galls and tea, and had a much more exalted chalybeate taste than the *Spaw*; and a small quantity of each having been kept for some time in bottles, in order to compare them, the *Pymont* was found to have retained its virtues much better, than the *Spaw*; and a glass of it being drank, it was found to be of a very agreeable relish, and to sit easy on the stomach.

The Cause of the variation of the Barometer, assigned in the History of the Royal Academy of Sciences for 1711, shewn to be insufficient; by Mr. Desaguliers. Phil. Trans. N° 351. P. 579.

THE paper is as follows; 'it appears by the barometer, that when it rains, or a little before rain, the air commonly becomes lighter: That it must rain, when the air becomes lighter, is easy to imagine; for, the imperceptible particles of water that swim about in the air in prodigious quantities, not being sufficiently sustained, when the air has lost a certain degree of its weight, begin to fall, and several of them joining together in the fall, form drops of rain; so when about half the air is exhausted out of the recipient of the air-pump (and consequently, the remaining air is as weak again as at first) something like a small rain falls; but why should the air become lighter? One might imagine, that in the place where it rains, it may have lost some of its weight and bulk, by means of the wind carrying away some part of it, but M. *Leibnitz* in a letter to the Abbot *Bignon* gives a new and more ingenious reason for it.

'He pretends, that a body, in any fluid, weighs with that fluid, and makes up part of its whole weight, so long as it is sustained therein; but if it ceases to be sustained, and consequently subsides, its weight no longer makes a part of the weight of the fluid, which thereby comes to weigh less; this may naturally be applied to the abovementioned particles of water, the weight of the air being increased, when it sustains them, and diminished, when it lets them fall; and as it may often happen, that the particles of water that are highest, fall a considerable time before they join with those that are low, the gravity of the air diminishes before it rains, and the barometer shews it.'

'This new principle of M. *Leibnitz* is surprising; for, must not a foreign body, whether sustained in a fluid or not, always weigh? Can it gravitate upon any other bottom than that which sustains the whole fluid? Does that bottom cease to carry an extraneous body, because this body falls? And is not that body all the while it is falling, part of the said fluid as to the weight? At that rate, whilst a chemical precipitation is made, the whole matter ought to weigh less, which yet has never been observed, and scarcely appears credible.

Notwithstanding these objections the principle holds good, when more narrowly examined: What sustains a heavy body is pressed by it; a table, for instance, that sustains a pound weight of iron is pressed by it, and is so only, because it sustains the whole action and effect of the cause of gravity (whatever it be) to push that lump of iron lower; if the table should yield to the action of that cause of the weight, or gravity, it would not be pressed, and therefore, would carry nothing; after the same manner the bottom of a vessel, which contains a fluid, opposes itself to all the action of the cause of gravity against the said fluid; if an extraneous body float therein, the bottom opposes itself also to the said action against that body, which, being in *equilibrio* with the fluid is in that respect really a part of it; thus the bottom is pressed both by the fluid and the extraneous body, and sustains them both; but if the body fall, it yields to the action of gravity, and consequently the bottom does no longer sustain it, neither will it sustain it, till the said body be come down to the bottom; therefore, during the whole time of the fall, the bottom is eased of the weight of that body, which is no longer sustained by any thing, but pushed down by the cause of gravity, to which nothing hinders it from yielding.

M. *Leibnitz* to confirm his notion, proposed the following experiment, *viz.* two bodies must be tied to the two ends of a thread, the one body heavier and the other lighter than water, yet such as both together may float in water; put them into a tube full of water, the tube being tied to one end of the beam of a balance, whose other end has a counterpoising weight; then if we cut the thread which ties the bodies together (that are of unequal weight) so that the heaviest may presently descend, he says, that in such a case the tube would be no longer in *equilibrio*, but its counterpoising weight would preponderate, because the bottom of the tube would be less press'd: It is plain, that the tube must be sufficiently long, that the falling body may not reach the bottom before the tube has time to rise: In chemical precipitations, the vessels are either too short, or what is precipitated falls sometimes too fast, and sometimes too slow; for then, the little bodies are always, as to outward appearance, in *equilibrio* with the liquor that contains them.

M. *Ramazzini*, the famous professor at *Padua*, to whom M. *Leibnitz* had proposed his experiment, made it with success, after some fruitless trials: M. *Reaumur* (to whom the

Academy

'Academy had recommended it) also made it with success: This is a new view in natural philosophy, which tho' it depends upon a well known principle, is very subtle and far fetched; and gives us just reason to suspect, that in subjects which seem to be exhausted, several things may still have escaped us.'

Mr. *Desaguliers* makes the following remarks on M. *Leibnitz's* new principle: Let AB Fig. 28. Pl. VIII. be the bottom of a vessel full of any fluid, whose top is either wider than the bottom as GH, narrower as EF, or equal to it as CD; the pressure of the fluid upon the base AB will be equal to the weight of CB, or of a cylinder or prism of the same fluid, made up of the area of the base, multiplied into the perpendicular height above it: If the fluid be equally dense every way as water, or of a density uniformly diminished as you go upwards; this proposition (called by Mr. *Boyle* the hydrostatical paradox) will hold good, and is demonstrated by all hydrostatical writers.

Let EF, as in Fig. 29. represent part of the surface of the earth, and GEFH a column of the atmosphere, whose height is GE the whole height of the air; let us suppose the vapours rising out of the earth to form themselves into two clouds A and B, and to settle in that place, where the air is of the same specific gravity with themselves; it is evident, that they will cause the air to rise so much higher, as their bulk amounts to, and will, therefore, make the surface which was at GH to rise up to IK, so that the bottom EF which was pressed by a column of air as GEFH, is now pressed by a higher column, as IEFK: Now if the clouds A, B, by any cause whatsoever, change their place, so as to come downwards, for instance, to CD, the height of the column IEFK will remain the same as it was, and therefore, the bottom EF will be pressed as before, by the foregoing proposition.

Corollary 1. If the clouds A, B, descend, and in their descent retain the same bulk as they had before, the surface IK will remain the same, and therefore, EF will be pressed as before.

Corol. 2. Whether a body be specifically lighter, or specifically heavier than a fluid, so long as it is detained therein, it will add to the fluid so much weight as the weight of an equal bulk of that fluid; wherefore, a body does not lose all that weight, which it added to the whole weight of the fluid, when it ceases to be sustained in the said fluid, contrary to M. *Leibnitz's* principle.

Scholium. If a cloud, by any cause whatsoever, become specifically heavier than that part of the air in which it swims, the excess

excess of its gravity above an equal bulk of air will cause it to descend, and accelerate its motion downwards; and then, indeed, it will lose of its weight by the resistance of the medium, till it come to an uniform, or sensibly uniform motion; but all the weight that it will lose, will only be the excess of its gravity above that of the air; for, with the rest of its weight it will still make up part of the weight of the air.

Experiment 1. Having with a weight in the scale C (as in Fig. 30.) of the balance A B counterpoised the long glass of water E I, Mr. *Desaguliers* let down with a horse hair the leaden weight W into the water, which from F G arose up to E H; and therefore, the water became heavier by the weight of a bulk of water, equal to the lead; having made up the counterpoise to the whole with another weight in C, he cut the thread of the plummet with a pair of fine scissars, and all the while the plummet was falling, the water rather descended than rose, and when the lead was at the bottom, the water overpoised, because it had then added thereto all the excess of the weight of the lead above an equal bulk of water, which by experiment is found to be $\frac{1}{12}$ parts of its weight: Had M. *Reaumur* and *Ramazzini* tried the experiment in this manner, the success would have been the same, but M. *Ramazzini* (as Mr. *Desaguliers* was informed by a gentleman who was present) tried it in the following manner.

Exper. 2. Making use of the above-mentioned machine, after Mr. *Desaguliers* had balanced the water and lead therein, he fixed to the end of the beam B (as in Fig. 31.) the thread of the plummet, which in the former experiment he held in his hand; this added to the weight, suspended at B, obliged him to put into the other scale a weight equal to $\frac{1}{12}$ parts of the lead, in order to recover the *equilibrium*; then cutting the thread or hair, the scale with the weights overpoised, whilst the lead was falling; but the *equilibrium* was restored, when it came to the bottom; so that the lead even then must have lost only its excess of weight above that of the water.

Exper. 3. Mr. *Desaguliers* tried the method propos'd by M. *Liebnitz* in the following manner; he took a cork C (as in Fig. 32.) weighing an ounce, and somewhat more than four times lighter than an equal bulk of water, and a ball of antimony W about four times specifically heavier than an equal bulk of water, and weighing four ounces; the cork laid upon the water in the vessel E A B D, rais'd the water from S S to G G, and added an ounce to the weight of the whole water; then suspending the ball of antimony by a string, and letting it hang

hang in the water at N, it rais'd the water from GG to HH, and so added another ounce to the weight of the water; then tying the ball of antimony to the cork, the cork had added $\frac{2}{3}$ of the weight of the antimony, which the hand had sustained before, and made it sink so, as to be almost covered, and rais'd the water to ik, Fig. 33 adding three ounces to its weight, after suspending this vessel of water upon the ballance, and a counterpoise at the other end, upon cutting the string, the vessel of water was rais'd up, and the *equilibrium* was not restor'd, till the antimony came to the bottom.

By observing, that as the cork (freed from the weight of the antimony) arose, and that during the fall of the body, the water sunk to *bb*, it appears, that this is in effect the same experiment as the former, and concludes no more: As to the real cause of the variation of the barometer, namely, the accumulation of the air by winds over the place where the barometer rises; and part of the air being blown away, where the mercury in the barometer falls, *Vide Dr. Halley's account of it in Phil. Trans. N° 181.*

In making the first experiment before the *Royal Society*, with a piece of lead, suspended by a thread, it was observable, whilst it was quite covered with water in the large tube (whose length was four feet) and in which it hung, not only that the end of the ballance, to which the tube of water with the lead in it was fix'd, did not rise when the thread was cut (to let the lead fall from the top to the bottom of the tube) as it must have done according to M. *Leibnitz's* principle, but that the said end of the balance began to descend from the time that the lead began to fall; therefore, in order to be assur'd, that it was not the plummet's rubbing against the sides of the tube in its fall, which caus'd that phenomenon, Mr. *Desaguliers* hung to the balance a long glass of three inches diameter, instead of the tube, and making the experiment as before, it succeeded in the same manner; the end of the ballance which carried the vessel of water sunk, as soon as the thread of the plummet was cut, tho' this glass was not above half so long as the tube.

When by holding the string Mr. *Desaguliers* drew the lead upwards and downwards in the water, there was no sensible alteration of the *equilibrium*; neither was it alter'd by cutting the string of a stone-plummet, because of the shortness of the glass, and the inconsiderable excess of specific gravity in the stone; for, the greater the difference is between the body, made use of

of in this experiment, and water, as well as the larger the body itself is, the better the experiment will succeed.

Hence it appears, that when a body specifically heavier than a fluid, is (by what cause soever) detained in any place of the said fluid, it adds as much to the weight of the whole fluid, as an equal bulk of the said fluid amounts to: And when the said body, by the action of its excess of specific gravity above the fluid, descends with an accelerated motion, so long as that motion is accelerated, the resistance of the fluid, which is, as the square of the velocity, takes off something of the whole weight of the body; but as much as the body loses, so much the water gains, over and above what was given it by its rising on account of the immers'd body.

A body, therefore, that falls in a fluid is so far from making the fluid lighter as it falls, that it makes it press more upon the bottom that sustains it when it is falling, than when it was at rest in the fluid: If the vessel of water be long enough for the falling body to come to an uniform motion before it reaches the bottom, the force impress'd on the water under the body will make it press the bottom, as much as if the body were actually at the bottom; the body in that case losing all its excess of gravity above that of the water, and the water gaining it.

Hence it follows, that a falling cloud, when it comes to an uniform motion, will not only add to the weight of the air as much as the weight of an equal bulk of air, but even as much as its whole weight amounts to, tho' it be specifically heavier than the air about it.

All the diminution of weight that can be allow'd in this case is this; if we imagine the air to have a smooth, regular surface, as we have at first suppos'd (or if that be not allow'd, we may take any imaginary surface of it above the clouds) when a falling cloud is diminish'd in bulk, as when it is changed into rain, the surface of the air will subside in proportion to that diminution, and, therefore, will weigh less, by so much as is the weight of a quantity of air, equal to the bulk that cloud has lost; but when the drops of rain after their acceleration (occasioned by their excess of gravity above that of the air) are come to an uniform motion by the resistance of the air, they restore to the air the weight it had lost: Now, this uniform motion being acquir'd in about two seconds of time, and the diminution of gravity in the air being insensible, when compar'd to near three inches of *Mercury* (for, such is the variation of the barometer with us) can noways be the occasion

of such sensible alterations therein, as happen some time before rain or fair weather. Add to this, that the whole quantity of rain that falls in *England* and *France*, in the space of one year, scarce ever equals two inches of *Mercury*; and in most places between the *Tropics* the rains fall at certain seasons in very great quantities, and yet the barometer shews there very little or no alteration.

An Account of an extraordinary effect of the Colic; by Mr. St. Andre. Phil. Trans. N^o 351, p. 580.

THE peristaltic motion of the intestines is by all anatomists suppos'd to be the proper motion of those cylindrical tubes: The use of this motion is to propel the chyle into the *vasa lactea*, and to accelerate the grosser parts of the aliment downwards, in order to expel them, when all their nutritive contents are extracted. This motion thus establish'd, it naturally seems to follow, that an inversion thereof (call'd for that reason an anti-peristaltic motion) should force the aliment, bile, pancreatic juice, and lastly the *feces* to ascend towards the mouth; the cause of this imaginary anti-vermicular motion is assigned to a stoppage of the intestine, or to a considerable length of it being entangled in the same manner, as the fingers of a glove are choak'd by inverting the glove in pulling it off; or like a silk stocking, which, when it is not garter'd, falls upon the foot, and is in a manner strangled, so that some force is requir'd to pull it up again.

This suppos'd, the anti-peristaltic hypothesis seems at first sight very natural, and solves most difficulties; for, if the vermicular motion accelerate the contents of the intestines downwards, the anti-vermicular, by the law of contraries, should force them upwards towards the mouth: Was this hypothesis as certain, as it is generally receiv'd, *Mr. St. Andre* would not offer to advance, that there is no such thing as an anti-peristaltic motion of the intestines, nor that the *miserere mei* is oftener a violent contraction of the abdominal muscles, than a stoppage or inversion of the intestines, as is suppos'd.

Supposing, that this disorder is a violent contraction of the abdominal muscles, as was already suppos'd, caus'd by the redundancy of the intestines or their contents; then comparing the symptoms of this disease with those of the different kinds of *hernia's*, we shall find by the analogy of the parts, by reason and by repeated experience, that the *chordapsus*, so call'd by

Celsus,

Celsum, is a disease in which the intestines and *omentum*, at other times the *pancreas* or spleen, nay, even the mesentery itself, are forced thro' the diaphragm into the *thorax*: All these tender parts, being strongly compress'd by the continual motion of this muscle, must, consequently, cause the same accidents, as in the *hiatonocela* or compleat *hernia*, there being no difference in these two cases; but that the first is a strangling of the intestine by the diaphragm, and the other a choking of the intestines by the abdominal muscles, as will appear from the following instance; a gentleman, that had come to *London* in perfect health, drank a great deal of new bottled oat-ale, after some pints of wine; these liquors fermented so violently in his stomach and intestines, that he was seiz'd with a violent colic the same night: In the morning an apothecary was sent for, who administer'd a glyster, and drew some ounces of blood to relieve the patient, who complain'd of a great pain in his left side: The glysters being repeated the following night, as also next morning, and the patient growing worse, the apothecary, without order of any physician, gave him a violent vomit, which operated eight or nine times; this added fuel to the fire, and the patient having from that time been in a desperate condition, two eminent physicians were call'd; who order'd that the glysters should be repeated; but they being all to no purpose, Mr. *St. Andre* was sent for about six hours before the patient died, who found him complaining of a violent pain in all the region of the *abdomen*, a frequent inclination to vomit, a great difficulty of breathing, together with a very slow pulse, and his belly very hard, tho' not swell'd.

This last indication made Mr. *St. Andre* conclude, that the disease was a violent contraction of the abdominal muscles, which had overcome the diaphragm, and that, probably, the intestines might be forced into the *thorax*: He was the more confirmed in this opinion from other examples of the like case; upon which he ordered a fomentation of hot milk, adding to every quart a drachm of liquid *laudanum*; which in these disorders gives great relief; but before it could be got ready, the patient expir'd in a violent convulsion.

Upon opening the body, Mr. *St. Andre* found the abdominal muscles so much contracted, that it was almost impossible to penetrate them with a sharp scalpel: Upon examination he found the stomach and some parts of the *duodenum* empty; but the *jejunum* and *ilium* were so much distended with the fermented oat-ale, that the *ilium* was four inches in diameter,

and the *colon* upwards of eight: The *ilium* was also pretty much inflamed in its inferior part, and all the valves of the *colon* were obliterated, by the great distention of that intestine: But the greatest disaster, was the dilatation made in the diaphragm, as he had before suppos'd, just upon the chink, that remits the intercostal nerve to the *viscera* of the *abdomen*, thro' which a portion of the *colon* was forced, as also the greatest part of the *omentum* and *pancreas*: These tender parts being compressed, were soon inflamed, upon which a mortification ensued; and a rupture of the pancreatic vein caus'd an internal hemorrhage, which fill'd all the left cavity of the *thorax*, inso-much, that the whole left lobe of the lungs was almost compressed under the *musculus scalenus*: The extravasated blood, tho' in considerable quantity, was not in the least coagulated.

Dr. *Haguenot* has in his *Memoir* prov'd the impossibility of the anti-peristaltic motion; and he is not far from truth, and what he says is certain, but it is surprizing, that the like case has not occur'd in his practice.

Two Northern Aurora's observ'd at Hone in Kent; by Mr. Barrel. Phil. Trans. N^o 351. p. 584.

ON Feb. 5. 1714 at 8 o'clock at night an *Aurora borealis* appear'd; it possess'd at least $\frac{2}{3}$ or $\frac{3}{4}$ of the horizon; it was low and shot out bright rays; and Mr. Barrel thinks it would have appear'd very bright, did not the moon shine at the same time, she being about five days old, and had not the *Aurora* disappear'd before the moon set.

Again, on March 30. following, there was another *Aurora borealis*: Mr. Barrel did not see it till past 9 o'clock, and then it was dim; and its highest part covered the lowest star in *Cassiopeia's chair*; it did not seem due north, but one point to the west; about 10 o'clock it shot out very bright rays, high, and tending somewhat towards each other; near 11 o'clock, there was, besides the northern brightness, a long streak, not very broad, that extended east and west, which beginning in the *Serpent's head* near *Hercules's club*, and covering *Arcturus*, proceeded near *Berenice's hair*, and so went over *Cor Leonis*, and thence to the *Canicula*, and ended a little beyond that star: It shone very bright at first, but faded away in about eight or nine minutes: If it had any motion (which Mr. Barrel could not positively say) it was southwards: He watch'd the next fit of brightness of the *Aurora*, and in about seven minutes, the eastern part of the streak, to wit, from the *Serpent's*

pen's head to near *Berenice's* hair, became visible again, tho' dim, and was quite effaced in four or five minutes more; and hitherto he did not perceive any change of its place.

An *Aurora borealis* seen at London; by Mr. Foulkes. Phil. Transf. N^o 352. p. 586.

MR. Foulkes being in the streets of London between eight and nine o'clock at night March 8. 1717, he perceived a light over the houses to the northerward, little inferior to that of the full moon when she first rises: Upon this he made all the haste he could into the fields, where he was for some time entertained with the sight of an *Aurora borealis*, attended with most of the phenomena of that very remarkable one of the 6. of March 1715-16.

The whole northern part of the horizon was in the same manner cover'd with somewhat resembling a very black cloud, from behind which there issued a considerable light, whose lower part was pretty well defined by the common edge of the cloud, but the upper died away more gradually; this upper limb of the light resembled the arch of a circle, whose higher point between 9 and 10 o'clock (when the meteor was most considerable) was elevated about 12 degrees, and bore, as Mr. Foulkes imagined, about 20 degrees westward of due north; it touch'd the horizon in the west, at the distance of about 65 or 70 degrees from the north, whence the whole intercepted arch of the horizon would have been nearly 100 degrees, had not some few degrees in the east been hid by clouds, which lay between the eye and the meteor.

The seemingly black cloud when he first saw it, ran nearly parallel to the horizon, and at the distance of six or seven degrees; but in about half an hour it changed its figure very much, sinking down in the north to about half its height, and rising in the west near as much; why he principally took notice of this, was, that the light issuing from behind it, did not change with it, but remained of the same figure, in what manner soever the cloud either approached or receded from different parts of its limb.

At first there arose some streams in the N. N. W. but of no considerable length, few of them passing five degrees above the arch, but beginning from behind the seeming cloud, so as to be about 12 degrees high in all; they were pointed at the extremity, and nearly vertical to the horizon: At times there was nothing but the arch to be seen, and that only resembling a common *aurora*; and

and again in an instant, by a sort of tremulous motion, several parts of it would appear converted into a vast number of parallel streams, for the most part very little higher than the arch itself. About 20 minutes before 10 o'clock, a small part of the arch, almost due north, grew remarkably brighter than the rest, and continued to increase for about half a minute, when there suddenly broke out some very tall streams of at least 60 degrees high, as he found by one in particular, which arose full north, and passing over the pole-star itself, reached some degrees beyond it: This was the most remarkable time of the appearance; some such streams or lances, tho' not so high, immediately shooting out of the place that radiated first of all, as did some more a good way to the east: They were all nearly perpendicular to the horizon, and most of them arose entirely from the black substance at bottom; tho' Mr. *Foulkes* saw some few that did not reach so low, appearing as if their lower parts had been broken off. Some of them were full as bright as any he saw in 1716; the axes (if they may be so called) of some of the tallest streams coming up very near to the colour of that pale fire, seen in some sorts of lightning: About this time the ground to the westward was all covered with an odd sort of mist, the same as that, from which in 1716 a great many people said, there proceeded a bad stench, which Mr. *Foulkes* did not at all perceive.

About 10 o'clock the phenomenon decreased very much, and so continued till after 11, only sending up at times two or three streams; at $\frac{1}{2}$ an hour after 11 it was again pretty much increased, and he saw it again emit some streams almost as considerable, as any he had seen before; the arch still continued, but not so entire, and from what he could judge, its middle was some degrees nearer the north than when he first observed it; the light continually abated till a quarter of an hour before 12, and then Mr. *Foulkes* left it; but he was informed, that it continued till towards day-break, but did not stream remarkably after he went away. Tho' he could not at this time see any stars thro' the black matter at bottom, he was sure it was not a cloud, tho' it bore the resemblance of one; for, when a real cloud (as several small ones did) came over any part of it, their difference was very plain.

Mr. *Foulkes* after this received two letters, one from *Wisbich* in the isle of *Ely*, the other from within 14 miles of *Bath*, both which took notice of it, tho' with no further particulars, than that they had seen the same light, tho' not so considerable, as in the beginning of *March* 1715-16.

Britain formerly a Peninsula; by Dr. Musgrave. Phil. Trans.
N^o 352. p. 389. *Translated from the Latin.*

Supposing *Britain* to have been a *Peninsula*, *Dr. Musgrave* proposes to examine, 1. Whether an *isthmus* or neck of land could not have been washed or worn away; and 2. Whether that between *Britain* and *France* really was not: The former is very strenuously denied by the learned *Isaac Vossius*; there being according to him no cause sufficient for producing such an effect; but the contrary will plainly appear from the following consideration; and first from that vast body of water, discharged from the *Atlantic Ocean* into our western sea, and that with such prodigious force, as is rarely found in any other part of our globe; both the shallows and streights of the western sea, increasing almost continually, must cause the waters thus impelled to mount in a surprising manner, and to beat against our supposed *isthmus*, so as to overflow, wear and wash it away in time; and thus, that *Britain* should become an island does not at all seem impossible, but on the contrary very probable: Secondly, the west wind is often so fierce and raging, after acquiring strength in the vast *Atlantic Ocean*, that it is scarce conceivable, with what fury it attacks the coasts of *Britain* and *France*; and it is very well known, that it commonly blows above half the year (which was also observed by *Julius Caesar*) and that very violently, especially in autumn, whence our *Michaelmas* storms; so that should it happen to concur with a strong tide, both the *Western* and *Severn Seas* would be greatly swelled; this *isthmus* would be strongly beat upon, and first its surface, consisting of flint and chalk, would be washed away; and that afterwards the remaining part of the *isthmus* should in the space of 2000 years and upwards, be worn away by the flux and reflux of the tide to 16 fathoms, its depth at this day, is by no means incredible.

To come now to the second thing, to wit, whether this *isthmus* was really worn away, or not: And 1. That remarkable ridge of land, in the streight itself shews that the land there was formerly much higher, but being continually washed away by the tides, for some thousands of years, was reduced to the state in which it is at this day; especially, if we consider, that it is a constant and infallible rule, that the more the bottom of the sea is worn or washed by its waters, the more level and even it becomes. 2. The steep white cliffs, consisting of chalk and flint, on the opposite shores of the streights, and answering to each other for six miles on each side plainly shew, that they were formerly

formerly separated by washing away the intermediate earth. 3. The state and condition of that tract of land, call'd *Rumney-Marsh* agrees very well with the supposition of an *Isthmus*; for, whilst the *Isthmus* remained, it must have been an obstacle to the tides, and consequently, have caused the overflowing of *Rumney-Marsh*, as being a plain low bottom; and that this marsh was formerly sea, appears from its strong bulwark, as also from the teeth and bones of a *Hippopotamus*, or some other sea-animal, dug up at *Chartham* in 1668, at 17 feet deep; *Phil. Trans.* N^o 272, 275. But an anchor, dug up thereabouts, shews it very evidently: After the *Isthmus* was broken thro', and all obstacles removed, the sea retired from *Rumney-Marsh* into its channel; whence what was formerly an estuary, is now a fertile plain, 20 miles long and 8 miles broad, and yielding very good pasture for cattle: Lastly, supposing, that there was formerly an *Isthmus* here, it is very easy to conceive, how wolves and other noxious animals might come into *Britain*; whereas on the contrary supposition it will be ridiculous to imagine, that they were transported thither in ships, for the conservation of their species.

Nor is it any objection, that there is no mention made of the breaking thro' of this *Isthmus* in any histories, either of the *Latins*, *Greeks*, or any other nation; for, how modern is the date of history, compar'd with that of the world? from the beginning down to the first history now extant, which is that of *Herodotus* there are about 3,500 years, and from *Noah's deluge* 1800; but in such a vast space of time, what conjunctions of causes might happen, and what changes might thereby be produced in our earth, is not easy to determine: Yet we must not allow that we have no hints of this event in history; for, what is plainer than this passage in *Virgil*.

— *Penitus toto divisos orbe Britannos.*

'Don't ye think (says the learned British Antiquary Jo. Twinn *de rebus Albionicis* p. 22.) that the word (*divisos*) may import the rending or breaking off one thing from another? And that *Virgil* knew its signification very well, and was well acquainted with antiquity, and had not forgot himself? On these words *Servius* says, 'because *Britain* was formerly joined to the Continent: Than which nothing can be more plain, but that the breaking thro' of this *Isthmus* was known to the ancients.'

There-

Therefore, from the whole the Dr. concludes, that *Britain* was not originally an island, but became such from a *Peninsula*, and that, as is probable, by the concurrence of some one or other of the more boisterous winds with the tides, and so breaking thro' the *Isthmus*.

Extracts from Mr. Gascoigne's and Mr. Crabtree's Letters, proving Mr. Gascoigne to have been the first Inventor of Telescopic sights of Mathematical Instruments; by Mr. Derham. Phil. Trans. N° 352. p. 603.

IN M. *De la Hire's* first part of his *Tabule Astron.* published in 1687, an invention, viz. the application of telescopic sights to astronomical instruments (which was undoubtedly our countryman Mr. *Gascoigne's*) is ascrib'd to M. *Picard*; M. *De la Hire's* words are to the following purpose, 'some few years ago M. *Picard*, a famous astronomer, and Fellow of the Royal Academy of Sciences at *Paris*, substituted telescopes, instead of plain sights in astronomical instruments; &c.' In which tho' it is not expressly said, that M. *Picard* was the inventor of this way, but only that he applied telescopes; yet because it implies, that it was M. *Picard's* invention, and is in effect claimed as such in M. *Auzout's* account of the telescopic micrometer in *Phil. Trans. N° 21*. Mr. *Derham* asserts Mr. *Gascoigne's* right to the invention from his own papers.

As to the invention of the micrometer, which M. *Auzout* claims as his, and M. *Picard's*, Mr. *Townley* has sufficiently prov'd it to be Mr. *Gascoigne's* in *Phil. Trans. N° 25*. and further to confirm it, Mr. *Derham* had by him the descriptions and draughts of that and some other instruments of that kind, in Mr. *Gascoigne's* own hand-writing.

And as Mr. *Gascoigne* was the first that measured the diameters of the planets, &c. by a micrometer; so he was the first that applied telescopic sights to astronomical instruments: In a letter to Mr. *Crabtree*, Jan. 25. 1642 (wherein he describes his micrometer, and shews his way of finding the refractions, the moon's parallax, and how he measured the diameters of the planets) Mr. *Gascoigne* tells him, how the measuring glasses, he had been speaking of, might be applied to a quadrant; 'if, says he, here (that is, in the distinct base) you place the scale that measures — or, if here a hair be set, that it appear perfectly thro' the glass — you may use it in a quadrant, for finding the altitude of the least star, visible by the

perspective wherein it is; if the night be so dark, that the hair or the pointers of the scale be not visible, I place a candle in a lanthorn, so as to cast light enough into the glass, which I find very helpful, when the moon does not appear, or when it is not otherways light enough.

In another letter, dated on *Christmas-eve* 1641 (wherein he describes the wheel-work of his micrometer, and shews how he could apply it to the taking of three points, and specifies his observations of the diameters of the sun and moon, and mentions a theory he had contriv'd of the sun, &c. says what pains he had taken in the dissection of the eye) he tells Mr. *Crabtree*, how he had applied his telescopic sights to a sextant; 'I shall shortly be furnish'd, says he, to try Mr. *Horrox's* theory of the moon; for, I am fitting my sextant for all manner of observations by two perspicille with threads: and I am also consulting my workman about making of wheels like β , γ , δ , & of diagram 3 (which is wanting in the letter) to use two glasses like a sector: If I once have my tools in readiness to my desire, I shall use them every night: I have fitted my sextant by the help of the cane, two glasses in it and a thread, so as to be a pleasant instrument, could wood, &c.'

In another letter (the date of which is worn out, but is in Mr. *Crabtree's* hand writing, call'd his tenth letter to him) 'I have, says he, given order for an iron quadrant of five foot, which will give me the thousandth part of one degree, which shall be furnished like my first scale, only I fear it will not be finish'd before the eclipse: I have caus'd a very strong ruler to be exactly made, and I intend to fit it with cursors of iron, with glasses in them and a thread for my sextant.'

From these passages from Mr. *Gascoigne's* letters, it appears, that he made use of telescopes on quadrants and sextants, as well as in his invention of the micrometer, as early as 1640.

What commendations these contrivances procur'd him, and what expectations they rais'd in some of the astronomers of that time, particularly in two of the most acute of that age, Mr. *Horrox* and Mr. *Crabtree*, may be seen in the same Mr. *Crabtree's* letters to Mr. *Gascoigne*, which Mr. *Derham* had also by him.

In Mr. *Crabtree's* second letter, dated *October 30.* 1640, after a very clear demonstration, that the solar spots are not planets at a distance from the sun, but something adhering to or very near the sun's body; and likewise after a no less clear demon-

demonstration of the errors of *Lansberg's Hipparchian* diagram, his lunar parallax, his doctrine of eclipses, and indeed his whole lunar astronomy, together with divers other curious matters; after this Mr. *Crabtree* goes on, 'something I am sure you were telling me concerning a way of observing the places of the planets by your glasses; but I have not a little lamented, that my time cut me so short, when I was with you, that I could not more fully ruminare and digest those strange inventions, which you shew'd me and told me of, &c. the multiplicity and variety of the novelties you shew'd me, so entirely distracted my thoughts into admiration, that I cannot now give my meditations any reasonable account of what I saw; but must intreat you in a few lines to rub up my memory, and tell me again what you shew'd me, and the extent of those your inventions; which I desire, that I might consider, and rejoice to consider, how much and wherein *Urania's* structure will grow to perfection by your assistance; and that what in me lies, I may help you to remember when and wherein your inventions and observations will be of most use; I should also desire you to inform me what bigness of a quadrant you conceive to be large enough for observation with your devices, &c. You told me, as I remember, you doubted not in time to be able to make observations to seconds; I cannot but admire it, and yet by what I saw, believe it; but long to have some farther hints of your conceit for that purpose: One means, I think, you told me, was by a single glass in a cane, upon the index of your sextant, by which, as I remember, you find the exact point of the sun's rays; but the way how I have quite forgot and much desire; your device for the exact division of a quadrant, by dividing 11 degrees into ten parts, I did then understand, but do not now fully remember; if it might not be too much trouble to you, I should intreat you to give me such a paper-demonstration thereof as you shew'd me, and two or three lines plainly of the use thereof, how to find those small parts: I lost the little paper, wherein I noted the moon's diameter, which we observ'd when I was with you; I pray you send it me, &c.

I cannot conceal how much I am transported beyond myself with the remembrance (of that little I do remember) of those admirable inventions which you shewed me, when I was with you: I should not have believed the world could have afforded such exquisite rarities, &c. Tho' modesty would forbid me to

request any thing (till you gave me leave) but what you please voluntarily to impart, yet the vehemence of my desire forceth me to let you know, how much I desire, and how highly I should prize any thing, that you should be pleased to communicate to me in those optic practices; could I purchase it with travel, or procure it for gold, I would not long be without a telescope for observing small angles in the heavens, nor want the use of your other device of a glass in a cane upon the moveable ruler of your sextant, as I remember, for helping to the exact point of the sun's rays, &c.

Thus was the most ingenious Mr. *Crabtree* transported with Mr. *Gascoigne's* devices, tho' at that time far less perfect than they were in a short time after; and no less affected was the incomparable Mr. *Horrox*, as Mr. *Crabtree* sets forth in his third letter, dated *December 28. 1640*, which has these words, 'my friend Mr. *Horrox* professeth, that little touch which I gave him of your inventions, has ravished his mind quite from itself, and left him in an extasie between admiration and amazement; I beseech you, Sir, slack not your intentions for the perfecting your begun wonders, &c. and then he thus proceeds, your diagrams for perspectives I have viewed again and again, and cannot sufficiently admire your indefatigable industry and profound ingenuity therein; I am much affected with the symbolical expressions of your demonstrations; I never used them before (but I will do) yet I understand them all at the first sight, and see well the truth of your demonstrations.'

In Mr. *Crabtree's* letter, dated *December 6. 1641*, at the beginning he says; 'that which you gave me a full projection of was above my hope; and if the screws keep an exact equality of motion forwards in each revolve, it is a most admirable invention, and with the other accommodations, I had almost said without compare; but that the divisions of a circle should be measured to seconds, without the limb of an instrument; or that distances, altitudes, inclinations and azimuths should be taken all at one moment, without the limb of an instrument likewise, and each to any required number of parts; or that the diameter of *Jupiter* should be projected in such prodigious measures as you speak of, &c. were enough to amuse and amaze all the mathematicians in *Europe*, and may indeed, be rather a subject of admiration than belief to any, that hath not known your former inventions to exceed vulgar, I had almost said human abilities; and for my part I must confess modesty so checks my ambitious desires, that I dare scarce hope
such

'such miracles should ever be produced in real practice to such exactness.' Then follows an account of the agreement of Mr. Horrox's theory of the moon with Mr. Gascoigne's observations; and likewise very curious reasonings and a disquisition about finding the parallax of the sun and moon, and their distance from the earth; in which he censures *Morinus* for his boasts, &c. and then says, 'that no man, that hath written of *Hipparchus*'s diagram, understood it fully, or described it rightly, but only *Kepler* and our *Horrox*; for whose immature death (which was suddenly and about the age of 25) &c.'

From the above cited passages of Mr. Gascoigne's and Mr. Crabtree's letters, relating to telescopic sights, it is very manifest, that long before the French gentleman's claims, our countryman Mr. Gascoigne had made use of those sights in his astronomical instruments, particularly in two or more sorts of micrometers, as Mr. Derham plainly finds, and in his quadrant and sextant.

Had Mr. Gascoigne lived longer we might have expected greater things from his pregnant and sagacious wit; for, he was scarce 20 years of age, when he held these correspondencies with Mr. Crabtree; and at the age of 23 he was killed at Marston-moor-battle on July 2. 1644. fighting for King Charles I.

An Account of the Dissection of a Child exceedingly emaciated; by Dr. Blair. Phil. Trans. N° 353. p. 631.

THIS child was five months old, and so emaciated, that it appeared rather to have decreased, than increased in bulk from the time of its birth; its whole body not weighing above five pounds: The skin and muscles of the abdomen were very thin, but the peritoneum was preternaturally thick; the ventriculus was more like an intestine than a stomach, its length being five inches and its breadth but one inch; its coats were thick and fleshy, and its cavity very inconsiderable; the pylorus and almost half the duodenum were cartilaginous, and something inclining to an ossification, so that no nourishment could have passed into the intestines, tho' the stomach had been capable of containing it, which makes it noways surprising, that the body was so emaciated; there were scarce any vestiges of the omentum to be seen, even at the bottom of the stomach, to which it usually adheres: The right lobe of the lungs firmly adhered to the ribs, and had three exulcerations, containing purulent matter; it was so very thin and compact, that it seemed, as if that lobe had never been used in respiration; the left lobe was of a more florid red, spongy and free from any adhesion.

Upon

Upon enquiring into the symptoms this child had been affected with, the mother told the Dr. that it seemed to be healthy till it was about a month old, when it was seized with a violent vomiting, and a stoppage of urine and stool; some time after, both these became more regular, but the vomiting still continued; it seemed to have a great appetite, taking what suck, drink, or other food was offered it with a kind of eagerness; but it immediately threw all up again; it had all along breathed freely, and had no cough, notwithstanding the exuberations above-mentioned; this confirmed the Dr. in the opinion that it had never breathed by the right lobe of the lungs.

There could be nothing more emaciated than this child was; and it seems to be worth considering, whether its illness might not be owing in a great measure to the want of the *omentum* (for, it seemed never to have had any) as also, whence it is, that this part is generally consumed in an atrophy, and in most hydropical cases, excepting where itself is more especially concerned.

Observations of the occultation of the fixed Stars by the Moon useful for finding the Longitude. Phil. Trans. N° 314. p. 693.

OF all the methods hitherto proposed for finding the longitude of places for geographical uses, none seems more adapted to the purpose than that by the occultations of the fixed stars by the moon, observed in distant places; for, those immersions of the stars, which happen on the dark semicircle of the moon, and their emersions from the same, are perfectly momentaneous, without that ambiguity, to which the observations of the eclipses of the moon, and those of *Jupiter's Satellites* are subject; besides, whilst the moon is horned, and her weaker light less dazzling, an ordinary short telescope, such as is found to be manageable on ship-board, suffices to observe those moments, even in the occultation of very minute stars; on which account, this way seems to bid fairest for finding the longitude at sea: But since it would be needless to enquire exactly what longitude a ship is in, when that of the port to which she is bound is still unknown; it were to be wished, that princes would cause such observations to be made in the ports, and on the principal head-lands of their dominions, as might once for all settle truly the limits both of the land and sea: This Work, however, being likely to be left to the care of private persons, it may not be amiss to give notice of the present opportunity of performing it, in this our northern hemisphere by means of the frequent appulses of the moon, to the more southerly

therly of the *hyades*, several of which she eclipses in each monthly revolution, and will continue so to do, during the years 1718, 1719 and 1720.

These stars are but three or four in all former catalogues, but Mr. Flamsteed in his *Catal. Britan.* increases them to 16; to these we have added three others somewhat smaller, viz. *c*, *i* and *n* as in Fig. 1. Plate IX. representing the *hyades*; in it the principal stars are denoted by Bayer's marks, and the rest by the letters of the *Italic* alphabet; their longitudes are fitted to the beginning of the year 1718, and being truly laid down, may serve to instruct the observer, when and where to look for them, when the moon is among them.

It appears by this scheme, that the distance between *a* and *e*, or *palilicium*, is about nine hours motion of the moon, in which time supposing her to pass from one to the other, she must eclipse *g* and *e*, and four or five of those about *θ*, and must apply very close with her southern limb to all those, which have about six degrees S. Lat. which would be a very entertaining sight: But if the times of the occultations of any one of these stars, or even of any two of them in the same night, be accurately observed under distant meridians, the difference of those meridians may be truly obtained thereby; especially, since the moon's parallax and all other parts of her theory, thereto requisite, are at present, sufficiently stated and known.

For the sake of such as are willing to make use of this method, we have added the places of all the *hyades*, fitted to the present time, and chiefly taken from the *British* catalogue, which being faulty in the stars we call *k* and *l*, we have here rectified them.

A Catalogue of the *hyades* in the beginning of the year 1718, as follows;

Names

Names of the Stars		Lo. Gem.	S. Lat.	M.
The star that preceeds γ <i>Tauri</i>	α	0 51 3	5 50 14	7
That in the <i>Bull's</i> nostrils, <i>Bayer's</i>	γ	1 50 54	5 46 23	3
That under γ	β	1 56 31	6 19 57	7
That in the root of the <i>Bull's</i> nose	ϵ	2 54 25	4 47 5	7
That between the nostrils and north eye of the <i>Bull</i>	δ	2 54 47	4 0 34	$\frac{1}{2}$
The star contiguous to this to the south	δ	3 10 33	4 9 4	6
The northern star of those proceeding θ	ϵ	3 17 21	5 41 50	8
The brighter southern star of these	ζ	3 25 32	6 2 44	6
The star that follows δ	ζ	3 35 2	3 43 27	5
The northern of the contiguous stars between the <i>Bull's</i> nostrils and <i>Palilicium</i>	θ	3 59 45	5 47 16	4
The more southerly star of the same	θ	4 0 11	5 52 55	4
The northern star of the two stars above θ	β	4 2 32	5 23 43	7
The more southerly star of the same	ι	4 7 44	5 36 40	8
The preceeding of three stars under θ in a streight line	κ	4 19 27	6 9 45	$\frac{1}{2}$
The middle star of the said three stars	λ	4 26 55	6 7 35	$\frac{1}{2}$
The northern of two stars following θ	m	4 30 26	5 37 49	7
The northern eye of the <i>Bull</i>	ϵ	4 30 31	2 35 58	3
The southern of those following θ	n	4 32 35	5 41 0	8
The following of the three stars under θ	o	4 45 55	6 0 55	7
<i>Palilicium</i> <i>Bayer's</i> α		5 50 20	5 29 50	1
The star that next follows this	p	6 17 35	6 3 20	7
The southern of the contiguous fol- lowing	q	6 30 34	6 19 19	6
The brighter and northern	σ	6 33 12	6 12 35	7

PLATE. VIII.

Fig. II.

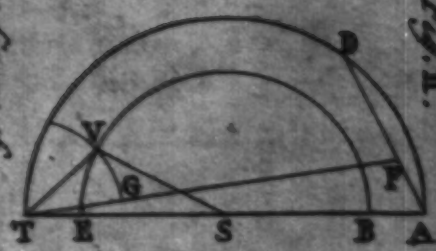


Fig. III.

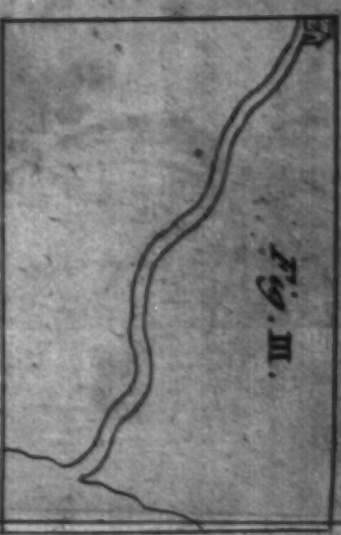


Fig. IV.

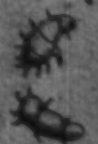


Fig. V.



Fig. VI.



Fig. VII.

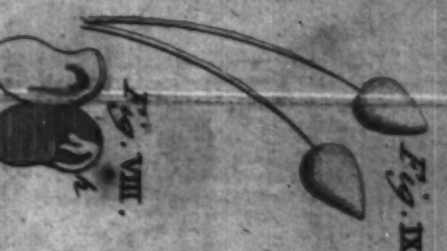


Fig. VIII.

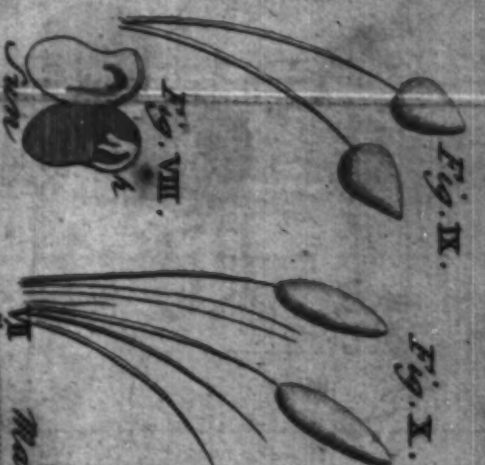


Fig. IX.



Fig. X.



Fig. XI.

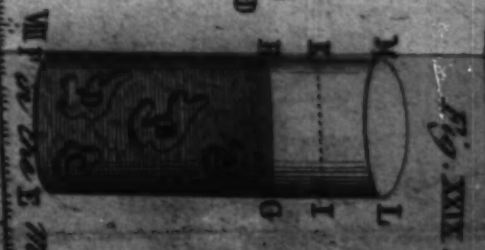


Fig. XII.

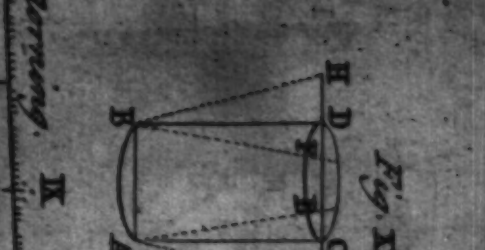


Fig. XIII.



Fig. XIV.



Fig. XV.



Fig. XVI.



Fig. XVII.



Fig. XVIII.



Fig. XIX.

Fig. XX.

Fig. XXI.

Fig. XXII.

Fig. XXIII.

Fig. XXIV.

Fig. XXV.

Fig. XXVI.

Fig. XXVII.

Fig. XXVIII.

Fig. XXIX.

Fig. XXX.

Fig. XXXI.

Fig. XXXII.

Fig. XXXIII.

Fig. XXXIV.

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Fig. XXXVI.

Fig. XXXVII.

Fig. XXXVIII.

Fig. XXXIX.

Fig. XL.

Fig. XLI.

Fig. XLII.

Fig. XLIII.

Fig. XLIV.

Fig. XLV.

Fig. XLVI.

Fig. XLVII.

Fig. XLVIII.

Fig. XLIX.

Fig. L.

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Fig. LX.

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Fig. LXXXXXXIX.

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Fig. LXXXXXXV.

Fig. LXXXXXXVI.

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A Roman Inscription near Lanchester; by Dr. Hunter. Phil.
Transl. N^o 354. P. 701.

FIG. 2. Plate IX. is an inscription, which was dug up in the Roman Castrum near Lanchester; it is very legible, and gives the Dr. reason to hope, an enquiry into the time of first fortifying this place would not be unsuccessful; especially, being able to fix the time of Gordian's repairing this fortress to the 243 year of Christ: We may reasonably ascribe the foundation thereof to the prudent administration of Julius Agricola; in the reign of Pl. Vespasian, about 169 years before.

In the second year of the Emperor Claudius A. D. 44. the Romans invaded Britain under the command of Aulus Plautius, in which expedition Vespasian, Sueton. Vespas. cap. 4. then Legate of the second legion, made a glorious figure, having been engaged in no less than 30 battles, and reduced two powerful provinces and upwards of 20 towns, together with the Isle of Wight: All these successes, tho' happily prosecuted in some of the following years and governments, could not frighten the natives into an entire submission; especially, as no progress had been made into the country of the Brigantes, till the advancement of Vespasian to the imperial throne, about 26 years after, A. D. 70. then the whole empire was delivered from the miseries of Nero's, and the short but lamentable devastations of the three succeeding reigns: Vespasian then resolved to push on the conquests he had begun in Britain; sent over thither choice armies, commanded by experienced generals; and the XX legion, which had acted seditiously in the preceeding troubles, was not without difficulty reduced to submit to Vespasian: Julius Agricola was constituted Legate, who, under the governour Petilius Cerealis, bore a considerable share in the successes against the Brigantes; Tacitus in Vita Agric. cap. 8. The same author afterwards cap. 17. adds, that he conquered the greatest part of the country of the Brigantes: Notwithstanding these advantages, the Dr. does not suppose, that the Romans did then penetrate so far into this province as Longovicum, situated so near the northern bounds of the Brigantes, that at present it is not above 12 miles distant from Corbridge, the Roman Curia, the chief town of the neighbouring people the Otadini.

The Dr. fixes on the second year of Julius Agricola's government for this work: Tacitus Vit. Agric. cap. 20. describes his conduct to the following purpose, 'Agricola as soon as it was summer, having got his army together, pitched upon the proper places for encamping, and in person examined the marshes

and woods; and in the mean time did not suffer the enemy to be at rest, that he might be the less exposed to their sudden excursions; and after he had sufficiently terrified them, he would then by his lenity allure them to peace; by which conduct a great many states submitted and gave hostages, upon which he built garrisons and fortresses round them'. This excellent conduct Tacitus farther confirms from the observation of others; 'for, says he, those skilled in these matters observed, that never any general chose his posts of advantage with better judgment, for, that no fortress built by Agricola was either taken by storm, delivered up by capitulation or deserted by the garrison.'

Agricola, having this summer reduced so large a tract, and finished so many fortresses, it cannot be expected all should be built with the most exquisite art, sufficient to perpetuate them.

To proceed to Gordian's repairs, whose historian Julius Capitolinus, tho' he has never once named Britain, yet gives so many hints of the excellent economy of his government, under the prudent administration of his father-in-law Misitheus, that the Dr. would fix this work to the third year of his reign, he having been before under the direction of the eunuchs and officers of the court, whom Capitolinus represents, in Misitheus's letter to Gordian, to have prostituted all employments to their own covetousness, and that of their mercenary creatures.

A new Genus of Plants; by M. Vaillant. Phil. Trans. N° 354.

P. 705.

A New genus of plants called *araliastrum* (of which the famous Nin-zin or Gin-seng of the Chinese is a species) has its flower A, as represented in Tab. 154. *Aralia* Instis. rei herbar. complete (that is, having a calyx) regular, polypetalous and hermaphrodite, standing on the ovary B; the ovary, which is crowned by a calyx, cut into several parts, becomes a berry D, in which there are for the most part two flat seeds, like a semicircle, which both together represent a sort of heart; add to this the stalk, which is single, ending in an umbel, each ray of which bears but one flower; above the middle of the stalk issue several pedicles (as on that of the *anemone*) on the extremities of which grow several leaves like rays, or like an open hand.

The species of this genus are 1. *Araliastrum quinquesolii folio, majus, Nin-zin vocatum, D. Sarrazin; Gin-seng, des lettres edifiantes & curieuses* Tom. 10. p. 172. 2. *Araliastrum quinquesolii folio, minus, D. Sarrazin; plantula Marilandica foliis in summo caule ternis, quorum unumquodque quinquesolium*

riam dividitur circa margines serratis, N° 36. Raii hist. 3. 638. 3. *Araliastrum fragrarie folio, minus*, D. Vaillant; *nasturtium Marianum Anemones sylvarie foliis, emphyllon, floribus exiguis*, Pluk. Mantiss. 135. Tab. 435. Fig. 7.

To shew wherein *Araliastrum* differs from *Aralia* (from which it takes its name) it is also proper to describe this last genus, such as M. Vaillant has established it in his demonstration for the year 1717, *Aralia*, as represented in Tab. 154. *Inst. rei herb.* 300. is altogether like the *Araliastrum*, as to the structure and situation of its flower; but its berry consists of five seeds, placed round an axis; moreover, its leaves are branched, almost like those of *Angelica*; and its stalks (which in some species are naked, and in others have leaves set alternately) bear each several umbels at their top, in the form of a bunch of grapes.

The species of *Aralia* are 1. *Aralia caule apophyllo, radice repente*, D. Sarrazin. *Christophoriana Virginiana Zarsa radicibus furculosis & fungosis, sarsaparilla Nostratibus dicta*, Pluk. Almag. 98. Tab. 238. Fig. 5. *Zarsaparilla Virginiensis nostratibus dicta, lobatus umbellifera foliis Americana ejusd.* Almagd. 396. 2. *Aralia caule folioso levi*, D. Sarrazin. *Aralia Canadensis* *Inst. rei herb.* 300. 3. *Aralia caule folioso & hispido*, D. Sarrazin. 4. *Aralia arborescens, spinosa*, D. Vaillant; *Angelica arborescens, spinosa, seu Arbor Indica, fraxini folio, cortice spinoso*, Raii hist. 2. 1798. *Christophoriana arbor aculeata Virginiensis*, Pluk. Almag. 98. Tab. 26.

All the species of these two genera, excepting the last of each of them, are common in Canada, whence M. Sarrazin first sent them to the Royal Garden at Paris in 1700: The inhabitants of that colony, and those of Virginia, call the first species of *Aralia* by the name of *Sarsaparilla*, because its roots have almost the same figure and virtues.

M. Sarrazin writes, that he had a patient who had been cur'd of an *anasarca*, by the use of a draught made of these roots; he also assures us, that the roots of the second species, well boiled and applied by way of cataplasm, are very good for curing old ulcers; as also the decoction of them, with which they bathe and syringe the wounds; he does not at all doubt, but the virtues of the third species (which he briefly describes) are the same with those of the second; its roots creep and send forth stalks, which commonly rise to the height of a foot and a half, and sometimes to two foot; the bottom part of them is rough, with reddish, stiff and prickly hairs; those stalks are set from the bottom to almost the top (which are divided successively into several naked

naked branches, charged with umbels) with branch'd alternate leaves, almost resembling those of *Podagria hirsuta* *Angelica folio et odore*, D. Vaillan; which plant is represented in the second Tome of Boccone's *Museum* and call'd *Cerastium rugosum Angelica folio, aromaticum*, Tab. 19. and in Rivini *Myrrhis folio podagriae*.

As for the Chinese *Gin-seng*, vide Phil. Trans. N^o 337.

Some Observations on the Eruptions of Mount Vesuvius;
by Mr. Berkeley. Phil. Trans. N^o 354. P. 708.

AP R I L 17. 1717. Mr. Berkeley with much difficulty, reach'd the top of mount *Vesuvius*, in which he observ'd a vast aperture, full of smoke, which hinder'd his seeing its depth and figure; he heard within that horrid gulph certain odd sounds, which seem'd to proceed from the belly of the mountain, as also a sort of murmuring, sighing, throbbing, churning and dashing of waves, as it were, and at times a noise like that of thunder or cannon, which was constantly attended with a clattering like that of tiles, falling from the tops of houses on the streets; sometimes, as the wind changed, the smoke became thinner, discovering a very ruddy flame, and the jaws of the pan or crater, were streak'd with red and several shades of yellow: After an hour's stay, the smoke, being mov'd by the wind, afforded short and partial prospects of the great hollow; in the flat bottom of which Mr. Berkeley could discern two furnaces almost contiguous; that on the left seem'd to be about three yards in diameter, glow'd with red flame, and threw up red hot stones with a hideous noise, which, as they fell back, caus'd the above-mentioned clattering.

May 8th in the morning, Mr. Berkeley went up a second time to the top of *Vesuvius* and found a different face of things; the smoke ascending upright, gave a full prospect of the crater, which, as he could judge, was about a mile in circumference, and 100 yards deep; a conical mount had been form'd since his last visit, in the middle of the bottom; this mount, he could see, was formed of the stones thrown up and fallen back again into the crater; in this new hill there remained the two mouths or furnaces already mentioned, that on the left hand was in the top of the hill, which it had formed round it, and rag'd more violently than before, throwing up every three or four minutes, with a dreadful bellowing, a vast many red-hot stones, sometimes in appearance upwards of a thousand

sand, and at least 300 feet higher than his head as he stood upon the brink; but there being little or no wind, they fell back perpendicularly into the *crater*, increasing the conical hill; the other mouth to the right was lower in the side of the same new formed hill; Mr. *Berkeley* could discern it to be fill'd with red-hot liquid matter, like that in the furnace of a glass-house, which raged and wrought like the waves of the sea, causing a short abrupt noise, like what may be imagined to proceed from a sea of quicksilver, dashing among uneven rocks; this substance would sometimes overflow and run down the convex side of the conical hill, and appearing at first red-hot, it changed colour, and hardened as it cool'd, shewing the first rudiments of an eruption, or an eruption in miniature, as it were; had the wind driven directly in the spectators faces, they had been in no small danger of being stiff'd by the sulphureous smoke, or being knock'd on the head by lumps of molten minerals, which were seen sometimes to fall on the brink of the *crater*, upon those shot from the gulph at bottom; but as the wind was favourable, Mr. *Berkeley* had an opportunity of surveying this odd scene for above an hour and a half together, during which time it was very observable, that all the vollics of smoke, flame and burning stones, came only out of the hole to the left, while the liquid matter in the other mouth wrought and overflow'd, as has been already describ'd: June 5. after a horrid noise, the mountain was seen at *Naples* to spew a little out of the *crater*; the same continued the 6th. The 7th nothing was observ'd till within two hours of night, when it began a hideous bellowing, which continued all that night and the next day till noon, causing the windows and as some affirm'd, the very houses in *Naples* to shake; from that time it belched out vast quantities of molten matter, to the south, which streamed down the side of the mountain, like a large pot boiling over; this evening Mr. *Berkeley*, returning from a journey thro' *Apulia*, was surpris'd, upon passing by the north side of the mountain, to see a great quantity of ruddy smoke lie along a large tract of sky over the river of molten matter, which was itself out of sight. The 9th *Vesuvius* raged less violently, and that night a column of fire was seen from *Naples* to shoot at times out of its summit. The 10th when it was thought all would have been over, the mountain grew very outrageous again, roaring and groaning most dreadfully. This noise in its most violent fits was like a mix'd confused sound, made up of the raging of a tempest, the murmuring

muring of a troubled sea, and the roaring of thunder and artillery; it was very terrible as heard in the farther end of Naples, at the distance of above 12 miles; this mov'd Mr. Berkeley's curiosity to approach the mountain; and for this end he and three or four others got into a boat, and were set a-shore at *Torre del Greco*, a town situated at the foot of *Vesuvius* to the south-west, whence they rode four or five miles, before they came to the burning river, where they arriv'd about midnight; the roaring of the *Vulcano* grew exceeding loud and horrible as they approach'd; Mr. Berkeley observ'd a mixture of green, yellow, red and blue, in the cloud over the crater; there was likewise a ruddy dismal light in the air over that tract of land, where the burning river flow'd; ashes continually showered upon them all the way from the sea coast; all which circumstances, together with the horror and silence of the night, made a scene the most uncommon and astonishing Mr. Berkeley had ever seen, and which grew still more extraordinary the nearer they came to the stream, resembling a vast torrent of liquid fire rolling from the top down the side of the mountain, and with irresistible fury beating down and consuming every thing that stood in its way: This mighty flood divided into two different channels, according to the inequalities of the mountain; the largest stream seem'd half a mile broad at least, and five miles long: As to the nature and consistence of these burning torrents, vide *Borelli's Latin* treatise of mount *Ætna*: Mr. Berkeley walk'd so far before his companions up the mountain along the side of the river of fire, that he was oblig'd to retire in great haste, the sulphureous steam having surpris'd him, and almost taken away his breath.

During their return, which was about three o'clock in the morning, they constantly heard the murmuring and groaning of the mountain, which at times would burst out into louder peals, throwing up huge spouts of fire and burning stones, which falling down again resembled the stars in rockets: Sometimes he observ'd two, at other times three distinct columns of flame, and sometimes one vast column, that seem'd to fill the whole crater; these burning columns and the fiery stones seem'd to be shot a thousand feet perpendicular height above the summit of the *Vulcano*. The 11th at night, he observ'd it from a terrace in Naples, to throw up incessantly a vast body of fire and large stones to a surprising height. The 12th in the morning it darkened the sun with ashes and smoke, causing

causing a sort of eclipse; this and the preceeding day, horrid bellowings were heard at *Naples*, whither part of the ashes had also reach'd. At night he observ'd it throw up flame, as on the 11th: On the 13th the wind changing, they saw a pillar of black smoke shot upright to a prodigious height; at night he observ'd the mount cast up fire as before, tho' not so distinctly because of the smoke. The 14th a thick black cloud hid the mountain, so as not to be seen from *Naples*. The 15th in the morning, the court and walls of the house in which Mr. *Berkeley* lodged at *Naples* were cover'd with ashes; in the evening a flame appear'd on the mountain thro' the cloud. The 16th the smoke was driven by a westerly wind from the town to the opposite side of the mountain. The 17th the smoke appeared much diminished, fat and greasy. The 18th the whole appearance ended, the mountain remaining perfectly quiet without any visible smoke or flame: A gentleman whose window look'd towards *Vesuvius* assur'd Mr. *Berkeley*, that this night he observ'd several flashes of lightening, as it were, issue out of the mouth of the *Vulcano*.

Mr. *Berkeley* observ'd the fluid matter rise out of the center of the bottom of the crater, contrary to what *Borelli* imagines, whose method of explaining the eruption of a *Vulcano* by an infected siphon, and the rules of hydrostatics, is inconsistent with the torrents flowing down from the very top of the mountain.

An Account of an extraordinary Tumour or Wen on the Cheek; by Dr. Bower. Phil. Trans. N° 354. p. 713.

IT is generally acknowledged, that the exact observation of internal diseases, and the faithful accounts of external tumours and extraordinary cases in surgery, have contributed very much to the advancement of physick: *Hippocrates* and *Galen*, and other ancient fathers of medicine have set us fair copies of this; and the moderns happily treading in their footsteps, have given us several instances of extraordinary cases in physick and surgery, and illustrated this subject by several curious observations and reflections.

One *Alexander Palmer* of the parish of *Keith* in the county of *Bamff*, in the north of *Scotland*, about 54 years of age, observ'd when he was about 27, a little hard swelling in the muscle of the lower jaw on the left side, without any hurt or visible cause; at first it went on slow, but afterwards more quick, and the older it grew, it still came on the faster, till
it

it became of a prodigious bulk and weight; from the first appearance of this tumour to its total excision, there were about 27 years; the patient had excessive pains and uneasiness therein, and at last it mightily extenuated him, tho' otherwise a strong robust man.

This excrescence was of the natural colour of the skin, and seemed to be an *Atheroma*, being a glandulous substance with several large blood-vessels therein, and had hair growing on it, as on the other parts of the body; it was almost round, and very hard, and as sensible as any other part of the body; for, when he was working in the fields, he accidentally made a large gash or wound therein with a sharp iron, which proved very painful, but being cur'd by a surgeon, after the manner of an ordinary wound, it still retained the cicatrice.

This excrescence having grown so big was attach'd to the *obliquus minor* or *inferior* under the left eye, to the ear and its muscles, and to the *deprimens* of the lower jaw; by reason of its bulk and weight, it could not hang down freely without some support; wherefore, it rested on the top of the shoulder, which made a considerable impresson therein; besides, it was supported by the man's hand in the day time, and laid on a pillow in the night time.

Some three or four days before the total excision of this tumour, the patient observ'd it begin to mortify at the lower end, which made him so uneasy, that he took a knife and cut off a good part of it: This occasioned a great hemorrhage; so that he reckoned, there was lost a *Scors* pint, or four pounds of blood, before it could be stopp'd: The patient, after so much pain, at last applied himself to Mr. Gordon, surgeon of the place, who made a total extirpation in the following manner; he made a close ligature, taking in the basis of the excrescence, and all the loose skin, and contracting it as much as possible, he cut it entirely off with a sharp razor; there gush'd out of the excrescence, after it was cut off and lay on the ground, as near as could be conjectured, two pounds of blood; for, it was nourish'd by several large blood-vessels, perhaps by some branches of the carotid artery, much enlarg'd, and other blood-vessels from the ear, the muscles of the eye and lower jaw above-mentioned: Three months after the excision of the tumour, the Dr. having cut off about a quarter of an inch broad of its basis, found therein four large blood-vessels, the basis, as it appear'd at that time, was five inches in diameter, which seems too large for the whole side of the face, so that
after

after the excision, the Dr. supposes the loose skin was turned backwards, which might make the basis appear so broad.

After all this loss of blood, the excrescence weighed full 19 pounds; so that before the patient's own incision, and this operation, it must have been several pounds heavier; which is a most prodigious weight to hang on such a place. This tumour was of a spheroidical figure, and measur'd 34 inches about by the longest way, and 28 by the broadest.

The hemorrhage, which was considerable, was stopp'd by the vitriolic powders and other astringents, and the ordinary dressing was us'd; so that this great cure was completed in six weeks time, and the patient entirely recovered. The lower lid of the patient's left eye continued depress'd a little, as did the same side of the mouth, which was occasioned by the great weight hanging on that side of the face; the head, at first, after cutting the tumour, inclined much to the right side, by reason the great weight on the left cheek was removed, but after that the patient perfectly recovered, it began to stand upright: Tho' the skin, and even a great deal of the muscular part of the cheek and lower jaw were cut away, yet they grew up again, and were of the ordinary colour of the skin, and like the other side of the face; so that there grew hair on that side of the face, as well as on the other, and which he commonly shav'd; and this was as surprising as any thing in this whole case.

An Account of an Experiment to prove an interspers'd Vacuum, by Mr. Desaguliers. Phil. Trans. N^o 354.

P. 717.

UPON dropping a guinea and a piece of fine paper, then a guinea and a feather together from the top of an exhausted glass-receiver about 20 inches high, they both fell to the bottom at the same instant of time: Now since the chief resistance of a medium (and indeed almost all of it) depends upon the quantity of its matter, vide *Sir Isaac Newton's Book 2. prop. 40.* therefore this diminution of resistance, whereby the feathers fell as soon as the guinea, shew'd a diminution of the quantity of matter, and consequently, prov'd an interspers'd Vacuum.

Some *Plenists* in England objecting against the shortness of the glass-receiver, as if the difference of time in the fall of the two bodies, which they affirmed to be real, could not be perceiv'd in such a glass; and some philosophers from abroad

affirming that in a glass-receiver seven or eight foot long, there would be such a manifest difference in the time of the fall of the said bodies, as to shew this experiment no proof of a *vacuum*; tho' at the same time some of the objectors well knew, that there could be no receivers made of half that length, and therefore, thought the experiment impracticable; to obviate this, Mr. *Desaguliers* contrived a machine for the purpose, which consisted of a strong wooden frame 15 foot high, that held the air-pump and four cylindric glass-receivers of about two foot long each, and six inches diameter; upon setting the first of these upon the air-pump, he laid on the top of it a brass-plate of seven inches diameter, that had an oiled leather fixed to it above and below, with a hole thro' the middle, of between four and five inches diameter; then on that plate he set the next receiver, with a like plate a-top; and after the same manner fixed the other two with plates between them; the upper receiver, being a little narrower at the neck, went into the hole of a board, whereby it was screwed down pretty hard on the other glasses, and fixed to the whole machine: On the top of this upper receiver he laid the brass-plate, wet leather and brass springs, which contained the bodies to be dropped.

When the receiver was full of common air before pumping, the guinea came to the bottom, just as the paper was about the middle of the second glass; but when the receiver was exhausted, the guinea and paper came to the bottom precisely in the same instant of time.

Mr. *Desaguliers* repeated this experiment before the *Royal Society*, first with two of the receivers, then with all the four, dropping a guinea and a small piece of paper together, and the success answered expectation: He afterwards made the four following trials; the whole machine being fixed, as above-mentioned, he first let fall a guinea and two papers, the one placed over and the other under it (before any air was exhausted) and the guinea came to the bottom, when the papers were only in the middle of the second glass from the top; then having laid a feather on the brass springs close by the guineas, he let them loose together; and the feather was fallen down only to the fourth part of the length of the first glass, or $\frac{1}{4}$ of the whole distance, when the guinea was got down to the bottom of the receiver: He then laid two papers and two feathers, one of each under, and the other over the guinea between the springs; and having exhausted so much of the air, as to bring up the mercury in the gage-tube within a quarter of an inch of the greatest height, to which

which it could be then raised by the pressure of the external air, he caused the bodies to fall all at once; and tho' the papers came down to the bottom at the same time as the guinea, yet the feathers being much lighter, wanted about three inches: But at last, having laid the papers, feathers and guinea, as before, he exhausted all the air; and then the feathers, as well as the papers, came to the bottom of the receiver at the same instant of time with the *Guinea*.

A small telescopic Comet seen at London on the 10th of June, 1717; by Dr. Halley. Phil. Trans. N^o 354. p. 721.

THAT the number of comets, traversing our solar system, is much greater than some, on account of the late rareness of their appearance, have supposed, may be collected from several small ones, which have within a few years been described in the *Memoirs of the Royal Academy of Sciences*, these diligent observers assuring us, that they discovered one in *Sept.* 1698, another in *Feb.* 1699, a third in *April* 1702, and again a fourth in *Nov.* 1707, none of which, as far as the Dr. could learn, were ever seen in *England*; all of them having been very obscure and without tails, by means of which comets do first usually shew themselves; and besides these, two other comets with remarkably long tails, the one in *Nov.* 1689, the other in *Feb.* 1702, passed by unobserved in these our northern climates, having great S. Lat. and their motions directed towards that pole; hence we may justly conclude, that the returns of comets are much more frequent than is commonly supposed, and that it is only contingent, that for these 35 years, no one of them has been seen and observed by our astronomers.

But there may be still a much greater number of these bodies, which, by reason of their smallness and distance are quite invisible to the naked eye; so that unless chance direct the telescope of a proper observer, almost to the very points where they are (against which there are immense odds) it will not be possible to discover them; and that this is not barely a conjecture, the Dr. gives the following instance.

On *Monday June 10. 1717*, in the evening, the sky being very serene and calm, he was desirous to take a view of the disk of *Mars* (then very near the earth, and appearing very bright) to see if he could distinguish in his 24 foot telescope, the spots said to be seen on him; directing his telescope for that purpose, he accidentally fell upon a small whitish appearance near the planet, in all respects resembling such a *nebula*, as the Dr. described in

Phil. Trans. N° 347, but smaller; it seemed to emit from its upper part a very short kind of radiation, directed towards the east, but northerly withall, which, considering its situation, was nearly towards the point opposite to the sun: The great light of the moon, then very near it, and also near the full, hindered this phenomenon from being more distinctly seen; but its place in the heavens was sufficiently ascertained from the neighbourhood of *Mars*, from whom it was but about half a degree distant towards the south-west, the difference of Lat. being somewhat more than that of *Long.* and *Mars* being at that time in $17^{\circ} 30'$ of *Sagittarius* with $3^{\circ} 48'$ S. Lat. the Dr. concluded its place was in $17^{\circ} 12'$ of *Sagittarius* with $4^{\circ} 12'$ S. Lat. or thereabouts; which may still be more securely determined by help of two small fixed stars he found near it, the more northerly of which he judged to have the same Lat. with it, and to follow it at about the distance of six minutes; the other star was about four minutes more southerly than the former, and about a minute in consequence thereof; the angle at the northern star was somewhat obtuse, of about 100 degrees, and the distance of the *nebula* from it was sesquialteral to the distance of the two stars, or rather a little more; Mr. *Williams*, Mr. *Thomas* and the Dr. viewed this appearance for above an hour, viz. from half an hour after 10 to near 12; and they could not be deceived, as to its reality; but the slowness of its motion made them at that time conclude, that it had none, and that it was rather a *nebula* than a comet.

However, suspecting, that it might have some motion, the Dr. attended next night, *June* the 11. at the same hours and in the same company, when with some difficulty, by reason of the thickness of the air, he found the two little stars, but the *nebula* could not be seen at that time, which was then imputed to the want of a clearer sky; but on *Saturday June* 15. the moon being absent, and the air perfectly clear, they had again a distinct view of the two stars, with an entire evidence, that there remained no footstep or sign of it, in the place where they had first seen this phenomenon, which they therefore now found to be a comet; and that being far without the orb of the earth, and in itself a very small body, it appeared only like a little speck of a cloud, such as would have scarcely been discerned in an ordinary telescope, much less by the naked eye.

Considerations on the change of the Latitudes of some of the principal fixed Stars; by Dr. Halley. Phil. Transl. N^o 535. P. 736.

DR. Halley, having had occasion to examine the quantity of the precession of the equinoctial points, compared the declinations of the fixed stars, delivered by *Ptolemy* in the third ch. of the 7. book of his *Almag.* as observed by *Timocharis* and *Aristyllus* near 300 years before *Christ*, and by *Hipparchus* about 170 years after them, that is, about 130 years before *Christ*, with what we now find; and by the result of a great many calculations, he concluded, that the fixed stars in 1800 years were advanced somewhat more than 25 degrees in Long. or that the precession is somewhat more than 90" per annum; but that with so much uncertainty, by reason of the imperfect observations of the ancients, that the Dr. has chosen in his tables to adhere to the even proportion of five minutes in six years, which from other principles we are assured is very near the truth; but while he was upon this enquiry, he was surprised to find the latitudes of three of the principal stars in the heavens directly to contradict the supposed greater obliquity of the ecliptic, which seems confirmed by the latitudes of most of the rest; they being set down in the old catalogue, as if the plane of the earth's orbit had changed its situation among the fixed stars, about 20' since the time of *Hipparchus*; particularly, all the stars in *Gemini* are put down, those to the northward of the ecliptic, with so much less Lat. than we find, and those to the southward with so much more southerly latitude; yet the three stars *Palilicium*, or the *Bull's eye*, *Sirius* and *Arcturus* do directly contradict this rule; for, by it, *Palilicium* being in the days of *Hipparchus* in about 10 degrees of *Taurus* ought to be about 15 minutes more southerly than at present, and *Sirius* being then in about 15 degrees of *Gemini* ought to be 20 minutes more southerly than now; yet on the contrary *Ptolemy* places the first 20 minutes, and the other 22 minutes more northerly in latitude than we now find them; nor are these the errors of transcribers, but proved to be right by their declinations, set down by *Ptolemy*, as observed by *Timocharis*, *Hipparchus* and himself, which shew, that those latitudes are the same, as those authors intended them; as to *Arcturus*, he is too near the equinoctial colure, to argue from him concerning the change of the obliquity of the ecliptic, but *Ptolemy* gives him 33 minutes more N. Lat. than he now has; and that greater latitude is likewise confirmed by the declinations

clinations, delivered by the aforesaid observers; so then all these three stars are found to be above half a degree more southerly at this time than the ancients reckoned them; when on the contrary, at the same time the bright shoulder of *Orion* has in *Ptolemy* almost a degree more southerly latitude than at present: What shall we say then? It is scarce credible, that the ancients could be deceived in so plain a matter, three observers confirming each other: Again these stars, being the most conspicuous in the heavens, are in all probability, the nearest to the earth, and if they have any particular motion of their own, it is most likely to be perceived in them, which, in so long a time, as 1800 years may shew itself by the alteration of their places, tho' it be entirely imperceptible in the space of a single century: Yet as to *Sirius*, it may be observed, that *Tycho Brahe* makes him two minutes more northerly than we now find him, whereas he ought to be above as much more southerly from his ecliptic (whose obliquity he makes two minutes and a half greater than we reckon it at present) differing in the whole four minutes and a half: One half of this difference may perhaps be excused, if refraction were not allowed in this case by *Tycho*, yet two minutes in such a star as *Sirius* is somewhat too much for him to be mistaken.

But a farther and more evident proof of this change is drawn from the observation of the application of the moon to *Palilicium* Anno Christi 509, March 11. when in the beginning of the night the moon was seen to follow that star very near, and seemed to have eclipsed it, ἐπιβalle γὰρ ὁ ἀστὴρ τὴν παρὰ τὴν διχοτομίαν μερὲς τῆς κυρτῆς περιφέρειας τῆ περιελισσόμενης μερὲς, i. e. stella appposita erat parti per quam bisecabatur limbus Luna illuminatus, as *Bullialdus*, to whom we are beholden for this ancient observation, has translated it: Now from the undoubted principles of astronomy, this could never be true at *Athens* or near it, unless the latitude of *Palilicium* were much less than we at this time find it, vide *Bullialdi Astr. Philolaica* p. 172.

But whether it were really true, that the obliquity of the ecliptic was, in the time of *Hipparchus* and *Ptolemy*, 22 minutes greater than now, may well be questioned; since *Pappus Alexandrinus*, who lived but about 22 years after *Ptolemy*, makes it the very same that we do, vide *Pappi Collect. lib. 6. prop. 35.*

An Account of some Experiments, together with an enquiry into the Cause of the ascent and suspension of Water in Capillary Tubes; by Dr. Jurin. Phil. Trans. N^o 335. p. 739.

THE following method was proposed to Dr. Jurin by an ingenious friend for making a perpetual motion from an observation of Mr. Hawksbee, and tho' it did not succeed, yet it gave occasion not only to rectify some mistakes of Mr. Hawksbee, but likewise to detect the real principle, by which water is raised and suspended in capillary tubes above the level; let ABC in Fig. 3. Plate IX. be a capillary siphon, composed of two legs AB, BC, unequal both in length and diameter, whose longer and narrower leg AB having its orifice A immersed in water, the water will rise above the level, till it fills the whole tube AB, and will then continue suspended; if the wider and shorter leg BC be immersed in like manner, the water will only rise to a certain height as FC, less than the entire height of the tube BC.

This siphon being filled with water, and the orifice A sunk below the surface of the water DE, the Dr's friend reasons thus; since the two columns of water AB and FC, by the supposition, will be suspended by some power acting within the tubes they are contained in, they cannot determine the water to move one way or the other; but the column BF, having nothing to support it, must descend, and cause the water to run out at C; then the pressure of the atmosphere driving the water upwards thro' the orifice A, to supply the vacuity, which would otherwise be left in the upper part of the tube BC, this must necessarily produce a perpetual motion, since the water runs into the same vessel, out of which it rises; but the fallacy of this reasoning appears upon making the experiments.

Experiment 1. For, the water instead of running out at the orifice C, rises upwards towards B, and running all out of the leg BC, remains suspended in the other leg to the height AB.

Exp. 2. The same thing succeeds upon taking the siphon out of the water, into which its lower orifice A had been immersed, the water then falling in drops out of the orifice A, and standing at last at the height AB: But in making these two experiments, it is necessary, that AG the difference of the legs exceed FC, otherwise the water will not run either way.

Exp. 3. Upon inverting the siphon full of water, it continues without motion either way; the reason of all which will plainly appear,

appear, when we come to discover the principle, by which the water is suspended in capillary tubes.

Mr. *Hauksbee's* observation is, as follows, let *ABFC* Fig. 4. be a capillary siphon, in which the water will rise above the level to the height *CF*, and let *BA* be the depth of the orifice of its longer leg below the surface of the water *DE*; then the siphon being filled with water, if *BA* be not greater than *CF*, the water will not run out at *A*, but remain suspended.

This seems, indeed, very plausible at first sight; for, since the column of water *FC* will be suspended by some power within the tube, why should not the column *BA*, being equal to, or less than the former, continue suspended by the same power?

Exp. 4. In fact, if the orifice *C* be lifted up out of the water *DE*, the water in the tube will continue suspended, unless *BA* exceed *FC*.

Exp. 5. But when *C* is never so little immersed in the water, immediately the water in the tube runs out in drops at the orifice *A*, tho' the length *AB* be considerably less than the height *CF*.

Mr. *Hauksbee* in his book of experiments has advanced another observation, namely, that the shorter leg of a capillary siphon, as *ABFC* must be immersed in the water to the depth *FC*, which is equal to the height of the column, that would be suspended in it, before the water will run out at the longer leg.

Exp. 6. From what mistake this has proceeded, the Dr. cannot imagine; for, the water runs out at the longer leg; as soon as the orifice of the shorter leg comes to touch the surface of the stagnant water, without being at all immersed therein.

The Dr. now proceeds a little farther and enquires into the cause of the ascent and suspension of water in capillary tubes: That this phenomenon is noways owing to the pressure of the atmosphere has been sufficiently proved by Mr. *Hauksbee's* experiments; and that the cause assigned by him, namely, the attraction of the concave surface, in which the suspended liquor is contained, is likewise insufficient for producing this effect, the Dr. thus demonstrates; since in every capillary tube, the height, to which the water will spontaneously ascend, is reciprocally as the diameter of the tube, it follows, that the surface containing the suspended water in every tube is always a given quantity; but the column of water suspended is, as the diameter of the tube; therefore, if the attraction of the containing surface be the cause of the water's suspension; it will follow, that equal causes produce unequal effects, which is absurd.

To this it may perhaps, be objected, that in two tubes of unequal diameters, the circumstances are different; and therefore, the two causes, tho' they be equal in themselves, may produce effects that are unequal; for, the lesser tube has not only a greater curvature, but those parts of the water, that lie in the middle of the tube, are nearer to the attracting surface than in the wider tube: But from this, if any thing follow it must be, that the narrower tube will suspend the greater quantity of water, which is contrary to experiment; for, the columns suspended are as the diameters of the tubes.

But as experiments are generally more satisfactory in things of this nature, than mathematical reasonings, the Dr. makes use of the following, which he takes to contain an *experimentum crucis*.

The tube CD, Fig. 5. is composed of two parts, in the wider part the water will rise spontaneously to the height BF; but the narrower part, if it were of a sufficient length, would raise the water to a height, equal to CD.

Exp. 7. This tube being filled with water, and the wider end C immersed in the stagnant water AB, the whole continues suspended.

Exp. 8. The narrower end being immersed, the water immediately subsides, and stands at last at the height DG (Fig. 6.) equal to BF: From which it is manifest, that the suspension of the water in the former of these experiments is not owing to the attraction of the containing surface; since if that were true, this surface being the same, when the tube is inverted, would suspend the water at the same height.

Having shewn the insufficiency of this hypothesis, the Dr. comes next to the real cause of that phenomenon, which is the attraction of the periphery, or section of the surface of the tube, to which the upper surface of the water is contiguous and to which it coheres; for, this is the only part of the tube, from which the water must recede upon its subsiding, and consequently, the only one, which by the force of its cohesion, or attraction, opposes the descent of the water: This likewise is a cause proportionable to the effect it produces, since that periphery and the column suspended are both in the same proportion, as the diameter of the tube: To put the matter out of all doubt, the Dr. confirms this assertion, as he has done the former, by actual experiment.

Let, therefore, EDC, Fig. 7. be a tube, like that made use of in *Exp. 7, 8.* excepting that the narrower part is of a greater

length; and let AF and BG be the heights, to which the water would spontaneously rise in the two tubes ED and DC .

Exp. 9. If this tube have its wider orifice C immersed into the water AB , and be filled to any height less than the length of the wider part, the water will immediately subside to a level with the point G ; but if the surface of the contained water enter never so little within the smaller tube ED , the whole column DC will be suspended, provided the length of that column do not exceed the height AF : In this experiment it is plain, that there is nothing to sustain the water at so great a height, but the contact of the periphery of the lesser tube, to which the upper surface of the water is contiguous; for, the tube DC , by the supposition, cannot sustain the water at a greater height than BG .

Exp. 10. When the same tube is inverted, and the water is raised into the lower extremity of the wider tube CD , Fig. 8. it immediately sinks, if the length of the suspended column DH be greater than GB ; whereas, in the tube DE it would be suspended to the height AF : From which it plainly appears, that the suspension of the column DH does not depend upon the attraction of the tube DE ; but upon the periphery of the wider tube, with which its upper surface is in contact: The two following experiments, which are in substance the same with *Exp. 9, 10.* shew the same thing in different manners.

ABC Fig. 9. is a siphon in whose narrower and shorter leg AB , if it were of a sufficient length, might be suspended a column of water of the height EF ; but the longer and wider leg BC will suspend no more than a column of the length GH .

Exp. 11. This siphon being filled with water, and held in the same position as in the Fig. the water will not run out at C the orifice of the longer leg, unless DC , the difference of the legs AB and BC exceed the length EF .

Exp. 12. If the narrower leg BC , Fig. 10. be longer than AB , the water will run out at C , if DC the difference of the legs exceed EF ; otherwise it will remain suspended.

In these two experiments it is plain, that the columns DC are suspended by the attraction of the peripheries at A , since their lengths are equal to EF , or to the length of the column, which, by the supposition, those peripheries can sustain; whereas, the tubes BC will sustain columns, whose lengths are equal to GH .

Tho' these experiments seem to be conclusive; yet the Dr. obviates an objection, which naturally presents itself, and which at first view may be thought sufficient to overturn his theory.

For, since a periphery of the tube *ED*, Fig. 7. can sustain no more than a column of the length *AR*, contained in the same tube; how comes it to sustain a column of the same length in the wider tube *DC*, which is so much larger than the former, as the section of the wider tube exceeds that of the narrower?

Again, if a periphery of the wider tube *DC* Fig. 8. can sustain a column of water in the same tube, of the length *BG*; why will it support no more than a column of the same length in the narrower tube *ED*? And the same queries may likewise be made with regard to the 11. and 12. *Exp.* the answer is easy; for, the moments of those two columns of water are precisely the same; as if the sustaining tubes *ED* and *CD* were continued down to the surface of the stagnant water *AB*, since the velocities of the water, where those columns grow wider or narrower are to the velocities at the attracting peripheries reciprocally as the different sections of the columns.

Exp. 13. From which consideration arises this remarkable paradox, viz. that a vessel being given of what form soever, as *ABC* Fig. 11. and containing any assignable quantity of water, how great soever; that whole quantity of water may be suspended above the level, if the upper part of the vessel *C* be drawn out into a capillary tube of a sufficient fineness. But whether this experiment will succeed, when the height of the vessel is greater than that to which water will be raised by the pressure of the atmosphere, and how far it will be altered by a vacuum, the Dr. is not hitherto perfectly satisfied.

Having discovered the cause of the suspension of water in capillary tubes, it will not be difficult to account for its seemingly spontaneous ascent; for, since the water, that enters a capillary tube, as soon as its orifice is dipped therein, has its gravity taken off by the attraction of the periphery, with which its upper surface is in contact, it must necessarily rise higher, partly by the pressure of the stagnant water, and partly by the attraction of the periphery immediately above that, which is already contiguous to it.

When this paper was reading before the *Royal Society*, the Dr. found, that the incomparable *Sir Isaac Newton* had been already acquainted with the above-mentioned principle; and several passages in the 31. query subjoined to the *Edis.* of his optics plainly shew, that he was master of it, when they were written, and *Mr. Machin* professor of Astronomy in *Gresham College* was likewise acquainted with it; and both furnished the Dr. with the following observation, that what he calls

periphery, or section of the concave surface of the tube, is really a small surface, whose base is that periphery, and whose height is the distance, to which the attractive power of the glass is extended.

Of the Motion of running Waters; by the Same. Phil. Trans. N^o 355. p. 748. Translated from the Latin.

WE often see, both in hydraulics and in applying its principles to the animal oeconomy, that the motion of water running thro' a hole in the bottom of a vessel, is compared with other powers; and since none has hitherto truly determined the quantity of this motion, hydraulic writers use to assume instead thereof the weight of a column of water incumbent on the said hole; and such as do this, never consider, that it is altogether impossible to compare any motion with a weight that is in a state of rest: But the motion of running water may be easily defined in the following manner.

Let SHAHS, Pl. 1. Pl. X. be an infinite surface of water, CC a circular hole made in the bottom, AB a perpendicular drawn thro' the center of the hole; SGCCGS a column or cataract of water running thro' the hole CC; SGC a curve, by whose rotation round the axis AB is generated the solid or cataract SGCCGS; for, when water descends freely and with an accelerated motion, as all heavy bodies do, it is necessarily contracted into a less bulk, as it acquires a greater velocity in falling, and runs out at the hole CC with that velocity, which it acquires by falling from the height AB. But the velocity acquired by a heavy body in its fall, from what *Galileus* has demonstrated, is in a subduplicate ratio of the height, from which it has fallen; wherefore, if any ordinate DE be drawn to the curve SGC, and DE be called y and AD, x , the velocity of the water in the section EE will be expressed by $\sqrt{x}$, and the product of that velocity into the section by $\sqrt{x} \times y^2$; and this product is as the bulk of water that passes thro' that section in a given space of time; and since the same bulk of water passes thro' each section of the cataract in a given time; consequently, that product will still be invariable, and $\sqrt{x} \times y^2$ will be $= 1$ and $x \times y^4 = 1$; and this is the equation of the curve SGC, whose part contained within the given vessel, was delineated, and its equation not obscurely hinted at by the Great Sir *Isaac Newton* in *Prop. 36. lib. 2. Princip.* who was the first that communicated to the learned world the true velocity of running water, and deduced it from its genuine principles;

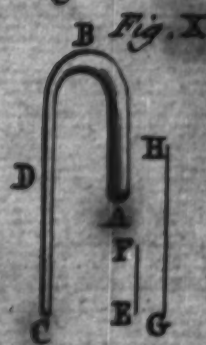
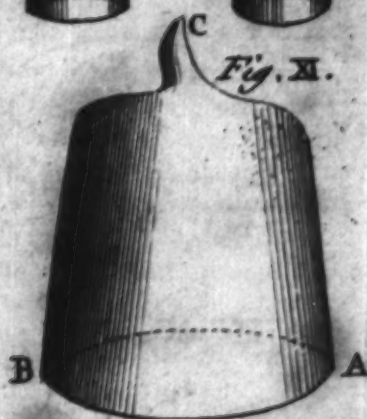
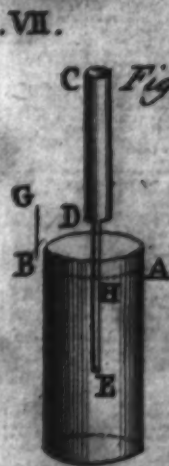
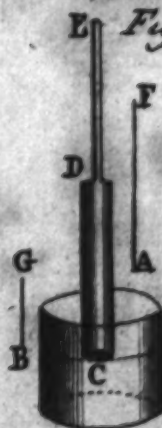
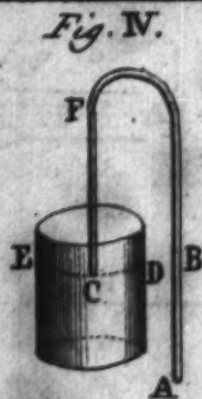
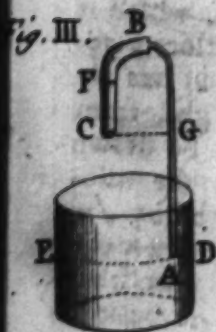
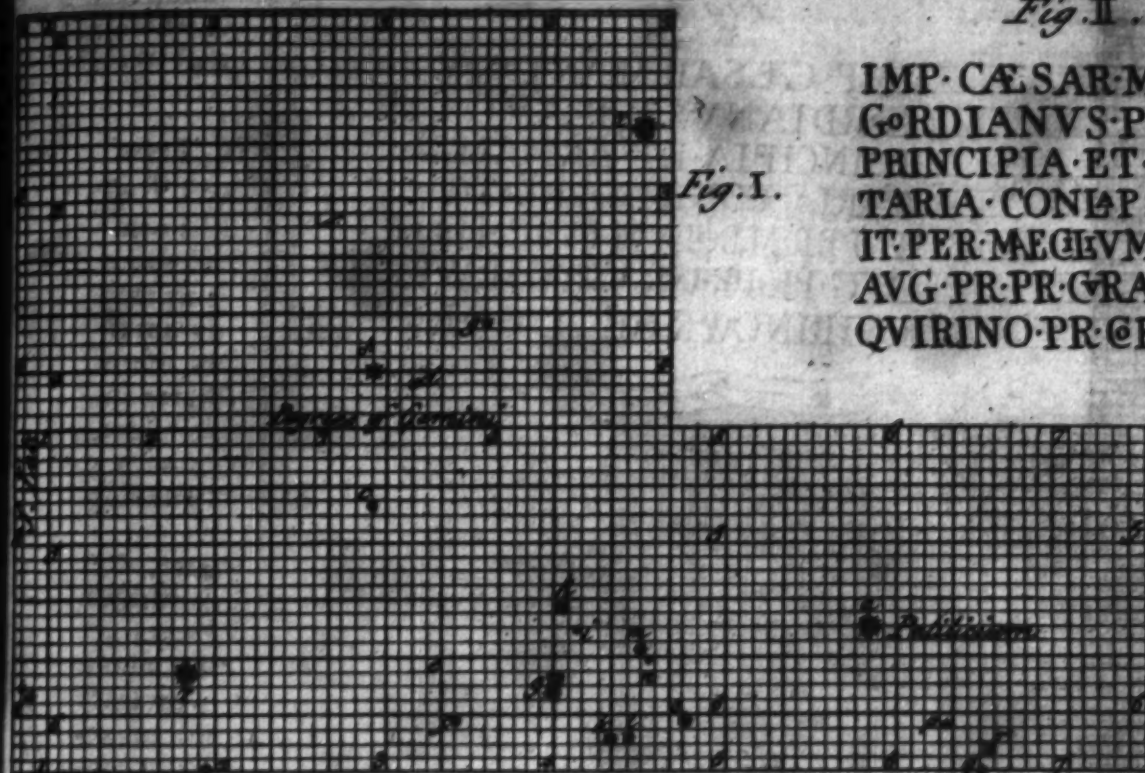
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Fig. II.

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IT. PER MEGLVM FVSCMLEG
AVG. PR. PR. GRANTE MAVR
QVIRINO PR. CHIL. GOR.

Fig. I.



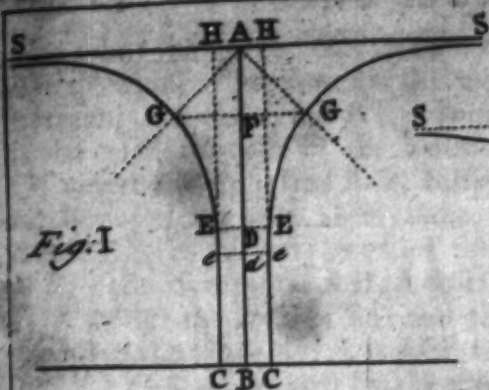


Fig. I.

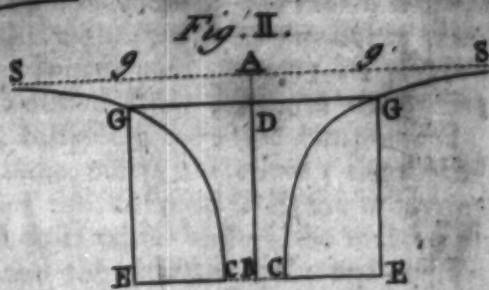


Fig. II.

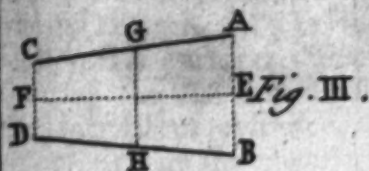


Fig. III.



Fig. IV.



Fig. V.

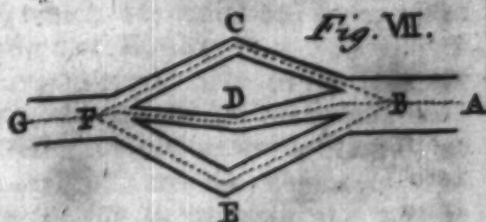


Fig. VI.

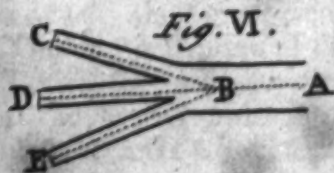


Fig. VII.

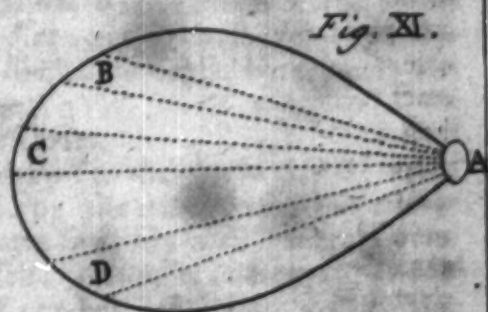


Fig. VIII.

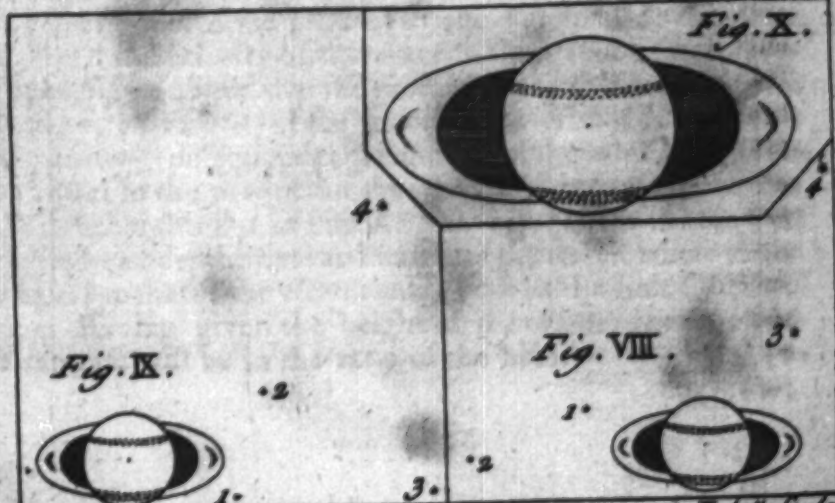
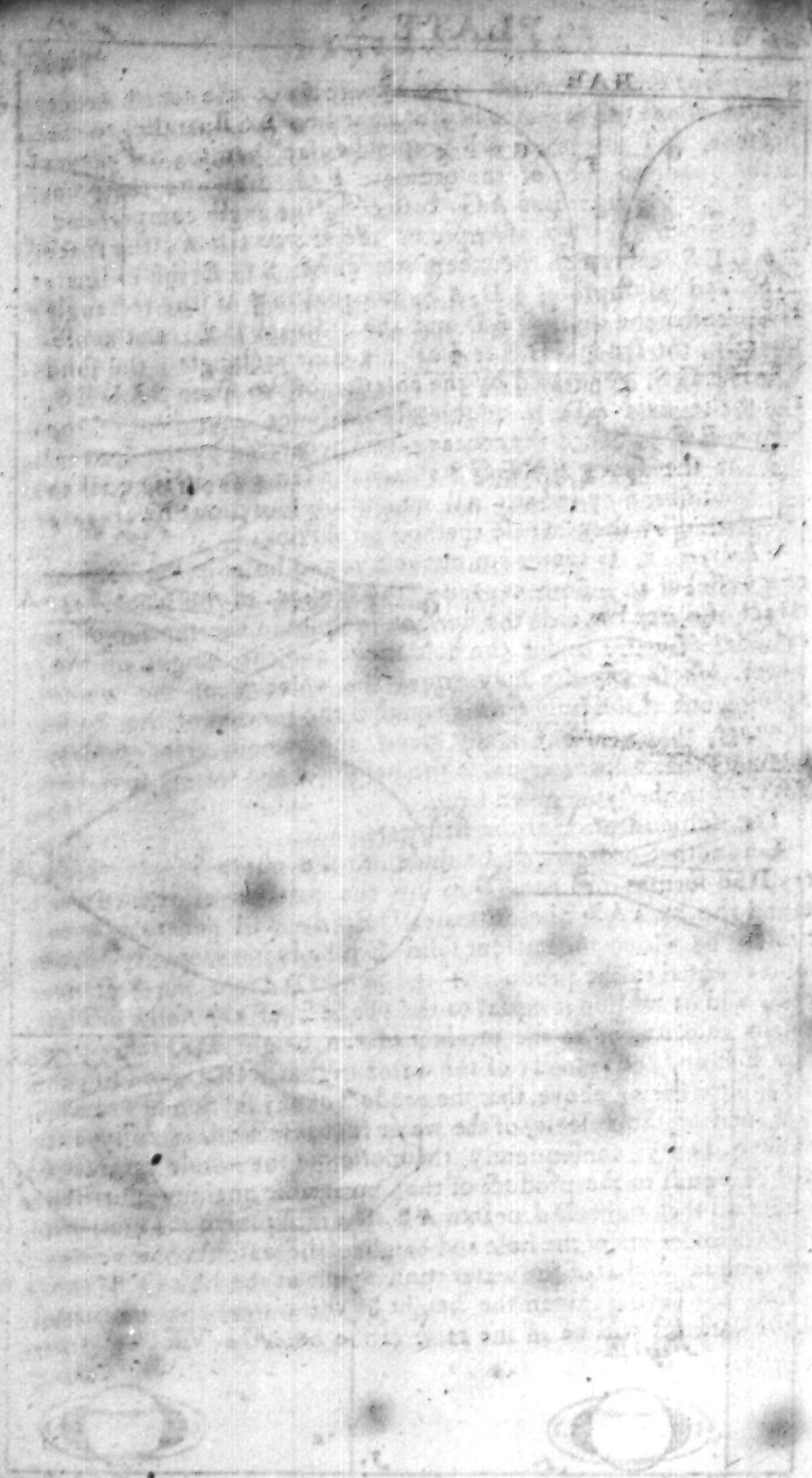


Fig. IX.

Fig. X.

Fig. XI.



ples; but the curve itself is an hyperbola of the fourth order, one of whose asymptotes is the right line AS , parallel to the horizon, and the other AB perpendicular thereto; its power is the quadrato-cube of the ordinate FG , drawn to the point G , where the right line AG , bisecting the angle comprehended between the two asymptotes meets the curve; the space $SADES$, contained between the curve SGE , the ordinate DE , and asymptotes AD , AS , is equal to $\frac{1}{4}$ of the rectangle HD , under the *abscissa* AD and the ordinate DE ; and consequently, the space SHE is $\frac{1}{4}$ of the same rectangle; the solid $SGEEGS$, generated by the rotation of the space $SADES$, about the axis AD , is double the cylinder incumbent on the section EE ; whence the concave solid generated by the conversion of the space $SHEGS$ about the same axis, is equal to the incumbent cylinder. All which are found out by an easy calculation by the inverse method of fluxions.

Theorem. 1. If water run out at a round hole in the bottom of a vessel of an infinite capacity, the motion of the whole cataract of water towards the horizon is equal to the motion of a cylinder of water under the hole itself, and the height of the water, whose velocity may equal the velocity of the water running out at the hole; or is equal to the motion of the bulk of water, that runs out in any given time, whose velocity may be such, that a space equal to the height of the water, may be run over in the same given time.

The demonstration of the first part.

Let another ordinate de be drawn to the curve SGC ; very near the former ordinate DE , by the rotation of the curve round the axis AB , the ordinates DE , de will generate two circles, by which the nascent solid $E E e e$ is intercepted; that solid is equal to the product of the height Dd into the section EE , and its motion is equal to the product of the solid itself into its velocity, or to the product of the height Dd , the section EE and the velocity of the water in that section; and since it has been shewn above, that the product of any section of a cataract, and of the velocity of the water in that section, is an invariable quantity; consequently, the motion of the whole cataract will be equal to the product of that invariable quantity into the sum of all the heights Dd , or into AB , that is, equal to the motion of the cylinder under the hole and height of the water, whose velocity is equal to that of the water running out at the hole $Q'E'D$.

Cor. 1. Having given the height of the water, the motion of the cataract will be in the ratio of the hole.

Cor.

deduced from the following general theorem.

Theor.

Cor. 2. Having given the hole, the motion of the cataract will be in the sesquiplicate ratio of the height, or in the triplicate ratio of the velocity with which the water runs out at the hole.

Cor. 3. Having given the motion of the cataract, the hole will be reciprocally in the sesquiplicate ratio of the height, or reciprocally in the triplicate ratio of the velocity.

The demonstration of the second part.

The bulk of water running out in a given time is to the cylinder under the hole and height of the water, as the length of the space the effluent water shall run over with an equable velocity in that given time, is to the height of the water; and since the velocity, which is ascrib'd to the bulk of the effluent water, is to the velocity of the cylinder reciprocally in the same ratio, the quantities of motion in both will be equal Q. E. D.

Cor. 1. Having given the height of the water and the bulk of the effluent water, the motion of the cataract is in the inverse ratio of the time, in which that bulk runs out.

Cor. 2. Having given the height and the time, the motion of the cataract is as the bulk of the water that runs out in that time.

Cor. 3. Having given the height and the bulk of the effluent water, the motion of the cataract will be as the ratio of the height.

Cor. 4. Having given the motion and height of the cataract, the bulk of the effluent water is in the ratio of the time.

Cor. 5. Having given the motion of the cataract, and the bulk of the effluent water, the height is as the time.

Cor. 6. Having given the time and motion of the cataract, the bulk of the effluent water will be reciprocally as the height.

Theor. 2. If AB , as in Fig. 2. be taken, which suppose to BD , as DC to BC , and the water running out at a round hole CC in the middle of the bottom of a given cylindrically vessel $GGEE$, which is always full, the motion of the cataract of water towards the horizon will be equal to the motion of the cylinder under the hole and height AB , whose velocity is equal to that of the water running out at the hole; or it will be equal to the motion of the bulk of water that runs out in any given time, and whose velocity is such that a space equal to the height AB may be run over in the same given time.

The demonstration of the first part.
 Let AS be drawn parallel to DG , and let the *Newtonian* curve SGC be supposed to be drawn with the asymptotes AS , AB , thro' the points G , C , in order to have the height of the water, the place of the water, that runs out, is to be supplied with a cylinder of water $ggGG$, descending with that uniform velocity, which is acquired by its descent from A to D , as the incomparable *Sir Isaac Newton* demonstrates in the above-mentioned proposition; the motion of the cataract $SSGG$ is equal to the motion of this cylinder by the preceding *Theor.* therefore, the motion of the descending water, since it is compounded of the motion of the cylinder of water $ggGG$, and of the motion of the cataract $GCGC$, is equal to the motion of the whole cataract $SGCCGS$, that is, by *Theor.* 1. to the motion of the cylinder of water under the hole and height AB , whose velocity is equal to that of the water running out at the hole Q . E . D .

The demonstration of the second part follows from the first.

Cor. 1. Hence by substituting the height AB , instead of that of the water, arise all the corollaries of the preceding proposition.

Cor. 2. If the vessel be of any other figure than cylindrical; or the figure of the hole be square, triangular, or any other but a round; or if the hole itself be not in the middle of the bottom; or if it be made even in the side of the vessel, the motion of the cataract will be the same, to wit, equal to the motion of the prism of water under the hole and height AB , whose velocity is equal to that of the effluent water; for, the same bulk of water will pass with the same velocity, as in the former hypothesis, both thro' the hole itself, and thro' each section of the cataract.

Cor. 3. If the diameter of the vessel have a very great ratio to that of the hole, the height AD may be neglected, and the height of the vessel itself may be assumed for that of the cylinder or prism of water.

The *Dr.* has hitherto considered apart, that particular case, in which water, by the force of gravity, runs out at a vessel; and the rather both because mathematicians do almost only apply this in treating of the *impetus* of fluids, and because he thinks the above explained property of the hyperbolic curve, whereby the cataract of falling water is formed, not unworthy the consideration of Geometricians; otherwise that case might be easily deduced from the following general theorem.

Theor.

Theor. 3. If water run thro' any full pipe ABCD as in Fig. 3 in the direction of the right line EF, to which both the orifices of the pipe, AB and CD are perpendicular, the motion of the water towards the orifice CD, or the motion of the impediment, which in the orifice itself stops the motion of the whole water is equal to the motion of the prism of water under any section of the pipe CH and the line of direction or the length of the pipe EF, because it move with the same velocity, with which the water flows thro' that section; or is equal to the motion of a bulk of water, which runs out at the pipe in any given time, and whose velocity is such that a space equal to the length of the pipe is run over in the same given time.

Case 1. Let the line of direction be any right line EF.

The first part is easily demonstrated in the same manner, as *Theor. 1.* for, the product of any section of the pipe CH, and of the velocity of the water in that section, is an invariable quantity.

The second part follows from the first.

Case 2. If the line of direction ABCDE, as in Fig. 4. consist of several right lines AB, BC, CD, EF, inclined to each other, the motion of the water will be the same; for, the motion of the water, in the whole compound pipe ABCDE, consists of the motions of the water in the several parts of the pipe AB, BC, CD, DE, added together; yet water, flowing according to the right line AB, and changing that direction into another, by which it is carried according to the right line BC, loses nothing of its motion; for, fluids do not follow the same laws, as are observ'd in the motion of solids, as often as their direction is changed; otherwise the motion of a fluid upon changing its direction into another perpendicular to the former direction, would be entirely stopp'd, which experiment does by no means shew: Moreover, water running out at the hole of a vessel, whether it be carried downwards, or according to the plane of the horizon, or streight upwards, has the same velocity; but if at any time it should be found, either by more subtil reasoning, or by experiment, that some diminution of the motion proceeds from the change of direction, regard is to be had thereto.

If the line of direction AB, as Fig. 4 be a curve, it is to be referr'd to this case; for, it may be conceiv'd as consisting of several small right lines.

Case 3. If the pipe AB, Fig. 6, be divided into several branches BC, BD, BE of equal length, the motion of the water will be found in the same manner, assuming for the line of direction the length ABD, consisting of the length of the principal pipe AB, and the length of any branch BD; for, it is the same thing, whether the water run from the principal pipe towards the branches, or from the branches towards the principal pipe; but if the branches be unequal, the motion of the water in each branch must be found out, taking for the line of direction the length, consisting of the length of each branch and that of the principal pipe: This is easily deduced from *case 2*.

Case 4. If the equal branches, into which the pipe AB, as in Fig. 7, is divided, unite again into one pipe FG, to find the motion of the water, you must take for the line of direction the whole length ABDFG, consisting of the length of the principal pipe AB, of any branch BDF, and of the united pipe FG: If the branches be unequal, the motion of the water in each must be found, and the sum of their motions added to the motion of the water in the united pipe: This follows from *case 2*, and 3.

Cor. 1. Having given the length of the pipe and any section thereof; the motion of the water will be in the ratio of the velocity, with which the water runs thro' that section.

Cor. 2. Having given any section, and the velocity of the water running thro' that section, the motion of the water will be as the length of the pipe.

Cor. 3. Having given the length of the pipe, and the velocity of the water in any section, the motion of the water will be in the ratio of that section.

Cor. 4. Having given the motion of the water and any section, the length of the pipe will be in the inverse ratio of the velocity.

Cor. 5. Having given the motion of the water and the length of the pipe, any section will be reciprocally as the velocity.

Cor. 6. Having given the velocity in any section, and the motion of the water, that section will be in a reciprocal ratio of the length.

Cor. 7. Given the length of the pipe, and the bulk of water running out in any determinate time, the motion of the water will be reciprocally at that time.

Cor. 8. Given the length of the pipe, and the time, the motion of the water will be as the effluent mass.

Cor. 9. Given the time and the mass of effluent water, the motion of the water will be as the length of the pipe.

Cor. 10. Given the motion of the water, and the length of the pipe, the effluent mass is in the ratio of the time.

Cor. 11. Given the motion of the water, and the effluent mass, the time will be as the length of the pipe.

Cor. 12. Given the time and motion of the water, the effluent mass will be reciprocally as the length of the pipe.

Cor. 13. If two masses of water, having a contrary motion, meet each other directly, and if both the superficies with which they impinge, as also the velocities, with which these superficies move towards each other be equal; and should one of the masses be equal to a small drop of water, and the other be the whole quantity of water contain'd in the ocean, or even an infinite quantity of water; it is possible, for this small drop of water not only to sustain the whole quantity of water in the ocean, or an infinite quantity of water, but after concurrence, to proceed in its motion in the same direction, and with the same velocity as before, and even repel the other towards the contrary parts; which is a surprising paradox in hydraulics.

Cor. 14. If a certain mass of water run from a wider into a narrower tube, thro' a pipe consisting of two cylindrical tubes of unequal diameters, and if the motion of the water be neither diminished nor increas'd in its course, as soon as the first part of the water enters the beginning of the lesser tube, it will immediately begin to run slower, and by its continual efflux from the wider into the narrower tube, the water will gradually be more and more retarded in the narrower tube, till the whole come into that tube: But the contrary will happen, when water runs from a narrower into a wider tube; which is another paradox in hydraulics: But water is suppos'd to cohere in all its parts: These two corollaries arise from case 1.

Cor. 15. From case 2. there is given a method of estimating the motion of the blood in any artery.

Cor. 16. Given any two arteries, that transmit an equal mass of blood, the *impetus* of the blood is greater in the more remote artery from the heart than in the nearer; which is a paradox in the animal oeconomy worthy observation.

Cor. 17. From case 3. there arises another paradox in the animal oeconomy; to wit, that the motion or *impetus* of the blood is greater in all the capillary arteries, taken together than

in the *aorta* itself; and in like manner it is greater in the capillary veins than in the arteries.

Cor. 18. From *case 4.* is deduced a method of determining the motion of the blood in any vein.

Cor. 19. From the same case is deduced a third paradox in the animal economy; to wit, that the *imperus* of the blood is greater in any vein, than in its corresponding artery, and consequently, that it is greater in the *vena cava* than in the *aorta*.

Problem 1. To find the motion of the air, expir'd from the lungs.

Let l be = the length of the whole wind-pipe from the mouth and nostrils to the extreme ramifications of the *trachea*; q = the quantity of air emitted from the lungs in a mean expiration; Q = the quantity of air in the strongest expiration; t = the time of the mean expiration; T = the time of the strongest expiration; hence by *Theor. 3. case 3.* the motion of the air, emitted from the lungs in a mean expiration is $= \frac{ql}{t}$ and in the strongest expiration $= \frac{QL}{T}$; that is, the

motion of the air, expir'd from the lungs, is equal to the motion of the bulk of air, emitted in one expiration, whose velocity is such as to run over the length of the whole wind-pipe in the time of expiration *Q. E. I.*

Alphonfus Borelli, by an experiment, defined the quantity of air, emitted in a mean expiration to be about 18 or 20 cubic inches; but this quantity is different not only in different persons, but even at different times in one and the same person; *Dr. Jurin* made the experiment in the following manner, he appended a weight to the lower part of a moistened bladder, and fitting to its upper part a glass tube about an inch in diameter, and stopping his nostrils, he gently inspir'd air into the bladder, for the space of three seconds of time, the weight in the mean time resting on a table; afterwards he immers'd the bladder with its included air and appended weight, into water, contained in a cylindrical vessel, carefully marking the height to which the water rose; then expressing the air out of the bladder, he immers'd it a second time with its weight into the water; upon which, the bulk of water, which, poured into the vessel, made up the height above marked, was easily found; having repeated the experiment 10 times, and adding together the several quantities found each time, their tenth part, or

the mean bulk of water poured into the vessel, was found equal to 35 cubic inches; which is the bulk of air contained in the bladder, and adding about a twelfth part more, or three cubic inches, by reason of the condensation of the air by the coldness of the water, it being in the winter time, there arise 38 cubic inches; moreover, there must somewhat be added, both on account of the pressure of the water upon the bladder, and also on account of the vapour, emitted with the breath, and turned into moisture, which must necessarily happen both from the coldness of the water and from the contract of the moistened bladder; therefore, he estimated the quantity of air emitted in gentle expiration in three seconds of time, in round numbers, 40 cubic inches: In the strongest expiration he emitted 125 cubic inches in one second of time; and by this sort of expiration, which, with a violent straining, he had continued almost till he was choaked, he emitted 220 cubic inches; whence it appears, (to mention this by the bye) that there remains much more air in the breast, than is emitted by one mean expiration: If therefore l be put = two feet; q = 40 cubic inches; Q = 125 cubic inches; r = 3"; T = 1"; the specific gravity of air to that of water, as 1 to 1000; a cubic foot of water = 1000 ounces *avordupois*; the mean motion of the air, expired from the lungs, will be equal to the motion of four scruples, nine grains weight, which runs over one inch in a second of time; or equal to the motion of 1 and $\frac{1}{2}$ gr. weight, which, in the same time may run over the length of five feet, seven inches; which is the velocity of the air emitted thro' the *larynx*, supposing the section of the *larynx* = $\frac{1}{2}$ of a square inch: The greatest motion of the air emitted from the breast is equal to the motion of about 1 and $\frac{1}{2}$ ounces weight, which runs over one inch in a second of time; or equal to the motion of 1 and $\frac{1}{2}$ gr. weight, which runs over 52 feet in the same time; which is the velocity of the air, emitted thro' the *larynx* in the strongest expiration.

Cor. 1. Given the quantity of air and length of the wind-pipe, the motion of the air is in the inverse ratio of the time of expiration.

Cor. 2. Given the bulk of the air and the time, the motion will be in the direct ratio of the length.

Cor. 3. Given the length and time, the motion will be as the quantity of the air.

Cor. 4. Given the motion and quantity of air, the length will be in the direct ratio of the time.

Cor.

Cor. 5. Given the motion and length, the bulk of the air will be directly as the time.

Cor. 6. Given the motion and the time, the bulk of the air will be reciprocally as the length of the wind-pipe.

Cor. 7. The motion of the air is in a ratio compounded of the quadruplicate ratio of any homologous diameter of the animal, and the inverse ratio of the time of expiration; or in a ratio compounded of the ratio of the weight of the whole animal, the subtriplicate ratio of the same weight, and the reciprocal ratio of the time; for, the weight of the animal, the cube of any homologous diameter and the bulk of the expelled air are all in the same ratio; and animal bodies are supposed to be machines alike formed.

Scholium. You are to conceive the length here assumed, either as the length of the wind-pipe, supposing all the branches of the *trachea* equal in length; or the mean length between the different lengths, if the branches are unequal.

Prob. 2. To determine the *impetus* or impression made upon the internal surface of the lungs by expiration.

Since re-action is equal and contrary to action; it must necessarily happen, that with what motion the air to be expired is urged by the internal superficies of the lungs, with just so much is the superficies of the lungs mutually repelled by the air; whence, by the preceeding *Prob.* the said *impetus* in a mean expiration is $= \frac{Q'}{T}$ and in the strongest expiration $= \frac{Q'}{T} Q. E. L.$

Hence, by supposing the same things as before, the mean *impetus* of the air upon the lungs is equal to the motion of about 1 drachm and $\frac{1}{4}$ weight, which in a second of time may run over one inch; or equal to the motion of a weight of 19 pounds, that runs over $\frac{1}{1642}$ part of an inch; which is the velocity of the air on the internal superficies of the lungs, supposing with Dr. James Kell, that the internal superficies of the lungs is equal to about 21900 square inches: But the greatest *impetus* of the air upon the lungs is equal to the motion of a weight of about 1 ounce and $\frac{1}{4}$, that runs over 1 inch in a second of time; or equal to the motion of a weight of 19 pounds, that runs over $\frac{1}{175}$ part of an inch in the same time; which is the velocity of the air on the superficies of the lungs in a strong expiration.

Cor.

Cor. 1. From this proposition arise the corollarics subjoined to the preceding Prob.

Cor. 2. A mean *impetus* incumbent on a part of the superficies of the lungs, equal to a section of the *larynx*, is the motion of a weight of $\frac{1}{1232}$ part of a grain, that runs over the space of 1 inch in a second of time; or the motion of a weight of 1 grain and $\frac{1}{1642}$ that runs over $\frac{1}{1642}$ part of an inch in the same time;

But the greatest *impetus* on an equal superficies is the motion of a weight of $\frac{1}{131}$ part of a grain, that runs over 1 inch; or the motion of a weight of 1 gr. and $\frac{1}{175}$ that runs over $\frac{1}{175}$ part of an inch each second of time.

Cor. 3. The *impetus* of the air, impressed upon the lungs in a mean expiration, is equal to the motion of a column of water, which runs over the space of 1 inch in a second of time, whose basis is the internal superficies of the lungs, and height, the $\frac{1}{68458}$ part of an inch; and the height of the column is $\frac{1}{7300}$ part of an inch in the strongest expiration.

Cor. 4. The *impetus* incumbent on a superficies equal to the greatest circle of a globule of blood, in a gentle expiration, is $\frac{1}{14}$ part of the weight of that globule; but in a strong expiration $\frac{2}{3}$ parts of the same weight, which runs over the space of an inch in a second of time.

Here the Dr. shews his manner of measuring the diameters of the globules of blood, since it may be of use to determine the magnitudes of other minute objects; he wound a fine and pretty long hair several times round a small needle, in such a manner, that all the circumvolutions touched each other exactly, as was discovered by the microscope; then taking with a pair of compasses the distance between the extreme circumvolutions on each hand, he applied it to a diagonal scale, and he divided the space thus found by the scale by the number of convolutions; whence he found the breadth of one convolution, or the diameter of the hair; afterwards cutting the hair into very small shreds or pieces, he strewed them on the plane of his microscope, on which he spread a little

a little blood in such a manner, as that the globules could be distinctly seen; upon viewing these shreds with his microscope, he found them in some places disposed so conveniently, that he could reckon, how many globules answered to the diameter of a shred; but the shreds were of unequal diameters, because the hair was more slender towards the extremity, than nearer the root; so that sometimes 7, or 8, at other times 12 or 13 globules answered to a transverse section of the hair; and having repeated both experiments several times, he at length estimated the mean diameter of the hair to be $\frac{1}{324}$ part of an inch, and the diameter of

a globule of blood $\frac{1}{10}$ part of the diameter of the hair, or $\frac{1}{3240}$ part of an inch.

Cor. 5. The *impetus*, which the internal superficies of the lungs sustains by the expiration of the air, is less than the motion of the gentlest dew that falls.

Scholium. In the solution of the two preceeding problems, the Dr. has not considered the obstacle to the egress of the air out of the lungs from the friction of the sides of the *trachea* and its branches; since that is very inconsiderable; nor does it seem, that it can be accurately enough estimated by any experiment; nor has he been very solicitous about making nice numeral calculations, having only proposed to exhibit a method of estimating (with greater certainty than seems to have been hitherto done) the forces with which the air in expiration acts upon the blood-vessels that creep along the internal superficies of the lungs; whence it may be known, whether these forces be equal for producing these effects, which are ascribed to them by some of the most learned medical authors.

Prob. 3. To determine the *impetus* of the blood in the *cava* near the right auricle of the heart; or the motions of the blood thro' all the veins and arterics, but the pulmonary.

Let q be = the quantity of blood thrown into the *aorta* in one systole of the heart; l = the mean length of the whole arterio-venous duct, having a regard to the longer and shorter branches; t = the time between two pulsations; hence by

Theor. 3. Case 4. the *impetus* sought is $= \frac{ql}{t}$, that is, the *impetus* of the blood in the *cava* is equal to the motion of the mass of blood, thrown out at one systole into the *aorta*, whose velocity

velocity is such, as with it the whole length of the veins and arteries may be run over, in the time between two pulsations.

Q. E. I.

If in the human body we suppose $q =$ two ounces averdu-pois; $l = 6$ feet; $t = \frac{3}{4}$; the *impetus* of the blood in the *vena cava* will be equal to the motion of a weight of 12 pounds, that runs over the length of an inch in each second of time; or to the motion of a weight of 2 pounds that runs over $\frac{1}{2}$ a foot in the same time; which is nearly the velocity of the blood in the *cava*; for, from Dr. Keill, we suppose the section of the *cava* to be $\frac{1}{4}$ of a square inch.

Cor. From this problem arise (*mutatis mutandis*) all the colloraries of the first problem.

Prob. 4. To determine the absolute motion of the blood in the *cava*; or the motion of the blood thro' all the veins and arteries, but the pulmonary, the resistance of the vessels being removed.

Let the natural velocity of the blood, be to that velocity with which the blood would flow, after removing all resistance, as 1 to α ; and since by Cor. of the preceeding Prob. and Cor. 1. of Prob. 1. the motion of the blood is in the ratio of the velocity, thence the motion sought will be $= \frac{\alpha q l}{t}$ Q. E. I.

If the proportion found by experiment by the above-mentioned Dr. Keill, be admitted, as near the truth, α will be $= 2.5$.

Whence, supposing the same things as in the preceeding Prob. the absolute motion of the blood in the *cava* is equal to the motion of a weight of 30 pounds that runs over the length of an inch in a second of time; or to the motion of a weight of two pounds, that runs over a foot and $\frac{1}{2}$ in the same time; with which velocity nearly, the blood being free of all resistance, will be carried thro' the *cava*.

Prob. 5. To find the motion of the blood in the pulmonary vein near the left auricle of the heart; or the motion of the whole mass of blood flowing thro' the lungs.

Besides the symbols made use of in Prob. 3. let λ be $=$ the mean length of the *arteria venosa*; whence, by Theor. 3. Case 4. the motion sought is found $= \frac{q \lambda}{t}$; that is, the motion of the blood, flowing thro' the lungs, is equal to the motion of a mass of blood, thrown into the pulmonary artery in one systole, with such

such a velocity, as to run over the length of the pulmonary veins and arteries in the time between two pulsations Q. E. I.

If in the human body λ be supposed $= 1$ foot and $\frac{1}{2}$; the motion of the blood in the lungs will be equal to the motion of a weight of three pounds, that runs over the space of an inch in a second of time.

Prob. 6. To determine the absolute *momentum* of the blood in the pulmonary vein.

By the same argument, as that made use of in *Prob. 4.* the motion sought is found $= 2.5 \times \frac{2\lambda}{t}$. Q. E. I.

And by supposing the same things as above, the absolute motion of the blood that flows thro' the lungs is equal to the motion of a weight of seven pounds and $\frac{1}{2}$ that in each second of time runs over the space of an inch.

Scholium. By Dr. Keil's experiment, the ratio is determined which the natural velocity of the blood thro' the *aorta* and its branches has to that resistance, with which the blood would flow thro' the same, if the resistance of the arteries and preceeding blood were removed; and Dr. Jurin has transferred that ratio to the blood flowing thro' the pulmonary artery; because, by either taking off or diminishing according to any ratio the resistance, the blood meets with in flowing thro' both arteries, the blood must necessarily be equally accelerated in both; for, unless that happened, both ventricles of the heart either will not be contracted in the same time, or will not discharge the same quantity of blood; either of which cannot possibly happen, without very much disturbing and endangering the whole machine.

Cor. To the three preceeding problems.

Hence follow the corollaries, *mutatis mutandis*, subjoined to the fifth *Prob.*

Scholium. To the four proceeding problems.

It is to be observed, that tho' the velocity of the blood, flowing both thro' the lungs, and also thro' the rest of the body, be not really equable, yet here it is supposed such, in order to find its mean motion. *General Scholium.* Should the numbers, which here and there are added to the symbols, appear less accurate to any one, he may easily, by finding by experiments numbers that approach nearer the truth, correct the above-mentioned examples of motion, either by the help of the propositions themselves or their corollaries.

An Account of the sinking of three Oaks into the Ground; by Mr. Le Neve. Phil. Trans. N° 355. p. 766.

ON Tuesday July 23. 1717, near the seat of Sir Charles Potts, Bart. (lying about mid-way between the market towns of *Holt* and *Aylsham*, and about seven miles from the coast near *Cromer*) in the parish of *Manington* in the county of *Norfolk*, first one single oak, with the roots and ground about it, was seen in the day time to sink into the earth, and not long after, at about 40 yards distance, two other oaks that were contiguous, sunk after the same manner into a much larger pit, being about 33 foot in diameter, whereas the former was not quite 18 foot; these, as they sunk, fell across, so that obstructing each other, only the roots of one of them reached the bottom, whereas the first three stood upright.

When the first tree sunk, it was observed, that the water boiled up in the hole; but upon the sinking of the two trees, the water drained off into the larger pit from the former, which continued dry; its depth to the firm bottom was nine foot three inches; and the tree that stood upright therein was three foot eight inches about, and its trunk about 18 foot long, half of which was within the pit: In the bottom of the larger pit, there was a pool of water about eight foot in diameter, whose surface was 11 foot three inches below the ground, and the trees in this pit were much of the same length with the other, but somewhat smaller; the one being three foot five inches about, and the other but two foot nine inches.

The soil, on which these trees grew, was gravelly; but the bottom a quick-sand over a clay, upon which were springs, that fed large ponds adjoining to Sir Charles's house, at about $\frac{1}{4}$ of a mile from these pits.

The nature of the soil seems to afford a reasonable conjecture at the cause of this odd accident; for, the springs running over the clay at the bottom of a bed of very fine sand, such as quick-sands usually are, may reasonably be supposed in a number of years to have washed away the sand, and to have thereby excavated a kind of subterraneous lake, over which these trees grew; and the force of the winds on their leaves and branches, agitating their roots, may well be supposed to have loosened the sand under them, and occasioned it to fall in more frequently than elsewhere; whereby in process of time the thin bed of gravel being only left, it might become unable to support its own weight, and that of the trees it bore: That this is not a mere conjecture, may

may appear from the boiling up of the water at first in the lesser hole, and its standing in the larger and lower; and if it shall be found, that it was a very windy day on which this accident happened, it will add much to the probability of this solution.

An accident not unlike this happened in *Fleetstreet, London*, by the defect of the arched roof of a very deep common sewer; the earth, falling gradually into the sewer, was carried away by it, so as not to obstruct the water; and the continual tremor of the ground, occasioned by the constant passing of carts and coaches, gradually shook down the earth, so as to leave a very large cavern, the top of which at length became so very thin, that one day a weighty cart having just past it, a large space of the pavement sunk in in the middle of the street, not without hazard to a coach then driving by.

A Rectification of the motions of the five Satellites of Saturn, with some accurate Observations on them; by Mr. Pound.

Phil. Trans. N° 355. p. 768.

ABOVE 30 years that great astronomer *M. Cassini* communicated to the world his discovery of two new satellites of *Saturn*, which made their number 5; as to which vide *Phil. Trans. N° 187*: Much about the same time *M. Huygens* made the *Royal Society* a present of the glasses of a telescope 125 foot long, with the apparatus for using them without a tube, by help of which we might have satisfied ourselves of the reality of these discoveries; but those in *England* that first tried to make use of this glass, finding, for want of practice, some difficulties in the management thereof, were the occasion of its being laid aside for some time: Afterwards it was designed for making perpendicular observations of the fixed stars passing by our zenith, in order to try, if the parallax of the earth's annual orbit might not be made sensible in so large a radius, according to what *Dr. Hook* had proposed long before; but in this we also miscarried, for want of a place of sufficient height and firmness, whereon to fix the object glass, so that it lay by neglected for several years.

In the mean time we could not but remark a great reserve in the *French* astronomers, in relation to these satellites, of which they have given us in their *Memoirs* no observations till very lately, nor have they seemed willing to shew them in their glasses to such as requested it; so that it might possibly occasion in some persons a suspicion of the reality of this discovery; and *Mr. Derham*, having borrowed the *Society's* long glass, could not thereby assure himself, that the small stars he sometimes found about *Sa-*

saturn, were really his satellites, their situation not agreeing with their places, as taken from the tables of their motions, in *Phil. Trans.* N^o 178. besides that, he wanted a sufficient height to raise the object glass (so as to view *Saturn* with advantage) above the vapours of the horizon: But in the *Memoirs* for 1714. M. *Cassini* the younger, has given us some observations, which clear up the point, and by shewing the errors of those first tables, has assured us, that we have seen the whole *Satellitium* of *Saturn*; the substance of these observations is as follows.

May 6. 1714. N. S. about midnight, *Saturn* being then stationary in $4^{\circ} 27'$ of *Virgo*, the fifth and outermost satellite was in its superior conjunction with the planet; and at the same time, the earth was nearly in the plane of this satellite's orbit, so that it appeared to pass very near *Saturn*'s centre: From hence and from some other preceeding observations, M. *Cassini* concludes, that the nodes of this satellite's orbit are in 4° of *Virgo* and *Pisces*; and that its inclination to the ecliptic is not much more than half that of the other satellites: Hence it should follow, that the ellipses it describes by its apparent motion about *Saturn*, when in *Gemini* and *Sagittarius*, are much flatter and nearer his body, than those of the other four, which he allows to move in the plane of the ring, and to have their nodes in 21° of *Virgo* and *Pisces*, with an inclination to the ecliptic of 31 degrees: To confirm this discovery, he produces another observation of his Father's, viz. that on May 31. 1683. N. S. about noon, the same satellite was observed in superior conjunction with *Saturn*, with less than one diameter of the ring N. Lat. *Saturn* being then in $11^{\circ} 48'$ of *Virgo*; so that the satellite wanted but $7^{\circ} 21'$ of completing 134 revolutions, in the interval of time between them: From these *Data* it was easy to settle the theory of this satellite.

As to the fourth or the *Huygenian* satellite, we find in the *Memoirs* for 1715 a very curious observation of it and the first of its kind, viz. that March 25. 1715. N. S. about 11^h p. m. this fourth satellite, then in apogee, did immerge behind the body of *Saturn*: With this emendation the place of this satellite may for the future be computed with sufficient exactness.

In *Phil. Trans.* N^o 187, the daily motion of the third satellite instead of $2^{\circ} 18' 4'' 50''$, should be $2^{\circ} 19' 41'' 50''$; as may be perceived by its period being determined in the aforesaid *Memoir* of 1714 to be $4^{\circ} 12^{\circ} 25' 12''$, that is, that it performs 400 revolutions in 1807 days: This satellite was observed by M. *Cassini* April 4. N. S. 10^h p. m. to have just passed its inferior conjunction with *Saturn*, and a perpendicular from it fell on the extremity

mity of the western *ansa*; so that at about 5^h p. m. it was applied to the center of the planet then in 5° 23' of *Virgo*; and consequently, in 5° 23' of *Pisces*: But in the beginning of 1685 N. S. the *epocha* thereof was 9° 39' of *Virgo*; so that from the noon of the last day of *December*, 1685, to *April* 4, 6^h 18' 1714, that is in 10320 days 6^h 18' there have been made 2284 $\frac{1}{2}$ revolutions of this satellite to the equinoctial; from which *data*, the tables of its motion are readily deduceable.

The radix of the penintime or second satellite, according to the aforesaid account, in the beginning of 1686, was 9° 26' of *Virgo*: But by the observations of M. *Cassini* made the nights both before and after this satellite was in its superior conjunction on *April* 4^d. 21' and $\frac{1}{2}$, 1714, N. S. that is, in 5° 21' of *Virgo*, where *Saturn* was at that time; so that *April* 4^d 22^h 12' an entire number of revolutions were performed since the *Epocha* of 1686, that is, in 10320 days, 22^h 12' which number can be no other than 3771, according to its period given in this *Memoir*, viz. 2^d 17^h 41' 22".

Lastly, the innermost or first satellite at the same time, viz. *April* 4. 21^h 30', 1714 N. S. was in its inferior conjunction nearly; and consequently in 5° 21' of *Pisces*: But the *Epocha* thereof for 1686 is 24° 50' of *Aries*, which place the satellite had pass'd 40° 31' at the time of the observation; it moves over this arch in 3^h 6'; wherefore, from the time of the *Epocha* to *April* 4. 16^h 24', 1714, or in 10320 days, 16^h 24' the satellite has perform'd 5467 revolutions, its period in this *Memoir* being determined to be 1^d 21^h, 18' 27".

Having by help of these later observations corrected the motions of the satellites, which, it was not possible for their first observer to settle truly, in the short interval before 1687; and having fix'd their *epocha*'s for the year 1718, there was a greater certainty of knowing where to expect them, and distinguishing them from each other, and from the small fix'd stars appearing with them; and Mr. *Pound* having by means of the steeple of *Wansled*, provided a *Gnomon* high enough for the purpose, and fitted a very commodious apparatus for using the Society's aforesaid long telescope, soon discover'd thereby all these five satellites, and communicated the following very curious observations.

April 21. 1718, at 10^h 40', the third and fourth satellites of *Saturn* were in *Apogeo*, a little pass'd their conjunction with *Saturn*; a perpendicular from the fourth satellite to the transverse axis of the ring, or line of the *ansa*, fell a little without

without the eastern *ansa*; and a line thro' the fourth and third satellites touch'd *Saturn's* eastern limb, as Fig. 8, Plate X.

The first satellite was northwards of the line of the *ansa* (and therefore, also in the *apogean* semi-circle) distant from the said line about as far as the extremity of the conjugate axis of the ring was from the center of *Saturn*, viz. $\frac{1}{4}$ of his semi-diameter nearly; and it was about a semi-diameter of the ring from the western *ansa*.

The second satellite was a very little southwards of the line of the *ansa* (and therefore, in the *perigean* semi-circle) upwards of a semi-diameter of the ring (or about the semi-diameter of the ring + the semi-diameter of *Saturn*) from the western *ansa*; and the third, first and second were in a straight line.

At 10^h 50' a perpendicular from the third satellite to the line of the *ansa* fell almost on the middle of the bright part of the eastern *ansa*, but somewhat nearer the center than the said middle.

April 22, 11^h 5' the four innermost satellites were all eastwards of *Saturn*; the second and fourth in the *apogean*, and the first and third in the *perigean* semi-circle; a line thro' the second and fourth touch'd the south-east limb of *Saturn*; a line passing thro' the third and the extremity of the conjugate axis of the ring, was parallel to the line of the *ansa*.

At 11^h 10' a perpendicular from the first satellite to the line of the *ansa* fell on the eastern extremity of the ring, as Fig. 9.

These distances and directions were taken by estimation only and not by an actual mensuration.

The fifth or outermost satellite being at this time near its greatest elongation eastwards, among several small telescopic stars Mr. Pound could not determine its position: but by observing its motion, some other nights before, he was fully satisfied, from the motions rectified as above, that there are five satellites of *Saturn*, as M. Cassini had long ago asserted.

In the bright part of each *ansa*, there was a darkish *ellipsis* nearer the outside than the inside of the ring, consisting of two rings near each other, as it were.

There appeared on the body of *Saturn* on the north side, besides the ring on the south side, a zone not so far from the center as the ring, and not much unlike the smallest of *Jupiter's* belts: These appearances as in Fig. 10. were first taken notice of by M. Cassini. vide *Phil. Trans.* N^o 128.

It is not to be expected, that these satellites, exceedingly minute in themselves, and so faintly illuminated, should appear when the air is but ordinarily serene, they requiring the medium to be not only very much defecated and limpid, but withal in perfect darkness, for which reasons it may well be understood, why the gentlemen of the observatory at *Paris* may sometimes have made a difficulty of undertaking to shew them upon demand.

Tables of the motions of Saturn's five Satellites, corrected according to later Observations, and render'd conformable to the Heavens. Phil. Trans. N° 356. p. 776. Translated from the Latin.

ABOUT the latter end of 1686 M. *Jo. Cassini* communicated to the *Royal Society* his discoveries of the motions of *Saturn's* five satellites, together with the *epocha's* of each, and their diurnal motion, as exhibited in *Phil. Trans. N° 187*. but since for 30 years after that, the *French* astronomers, who were the only proper persons, had given no observations at all of them; and since besides, by reason of the too short interval of time, the celebrated discoverer could but loosely determine the periods of the satellites, especially the innermost, the errors of the tables could not be corrected before these observations, mentioned at the latter end of the preceeding *Transaction*, were publish'd in the *Memoirs* of the *Royal Academy of Sciences* at *Paris* for the year 1714.

Having by the help of these observations made some emendation of the motions, we at length here in *England* discover'd with *Huygens's* telescope the whole *Satellitium* of *Saturn*, and by the help of Mr. *Pound's* accurate observations the following tables were made conformable enough to the heavens; to wit, by adding to the annual motion of the innermost satellite $2^{\circ} 9'$ and to that of the innermost but one, $3^{\circ} 25'$ still retaining M. *Cassini's epocha's* to the year 1686, and likewise by encreasing the annual motion of the outermost satellite by $9'$, and taking away 16° from the *epocha*, which in the said N° 187 instead of $16^{\circ} 19'$ of *Pisces* should be $16'$ of *Pisces*; we found the annual motion of the *Huygenian* satellite slower by $6'$: We were likewise oblig'd to make the tables of the third satellite anew (only retaining the *epocha*) because its diurnal motion was erroneous in the said *Transaction*.

A Table of the mean motions of *Saturn's* middle satelite, discovered by *Cassini*, in 1671

begin of A. C.	Epoch.	Year	Mean mot.	D	Mean mot	H.	Mean mot.	M.	Mean
1681	8 48	1	4 4 43	1	6 10 42	1	0 7 57	31	4 6
1686	8 13 4	2	8 9 25	2	0 21 24	2	0 15 53	32	4 14
1701	6 34	3	0 14 8	3	7 2 6	3	0 23 50	33	4 22
1714	9 57	4	10 29 33	4	1 12 47	4	0 31 47	34	4 30
1715	14 39	5	3 4 16	5	7 23 29	5	0 39 44	35	4 38
1716	11 19 22	6	7 8 59	6	2 4 11	6	0 47 40	36	4 46
1717	4 47	7	11 13 42	7	8 14 53	7	0 55 37	37	4 54
1718	9 30	8	9 29 6	8	2 25 35	8	1 3 34	38	5 2
1719	14 13	9	2 3 49	9	9 6 17	9	1 11 31	39	5 10
1720	8 18 55	10	6 8 32	10	3 16 59	10	1 19 28	40	5 18
mon. com. year	mean mot.	11	10 13 15	11	9 27 41	11	1 27 24	41	5 26
		12	8 28 40	12	4 8 22	12	1 35 21	42	5 34
		13	1 3 23	13	10 19 4	13	1 43 18	43	5 42
Jan.	0 0 0	14	5 8 5	14	4 29 46	14	1 51 15	44	5 50
Feb.	5 1 38	15	9 12 48	15	11 10 28	15	1 59 11	45	5 58
Mar	3 1 10	16	7 28 13	16	5 21 10	16	2 7 8	46	6 5
Apr.	8 2 48	17	0 2 56	17	0 1 52	17	2 15 5	47	6 13
May	6 23 44	18	4 7 39	18	6 12 34	18	2 23 1	48	6 21
June	11 25 22	19	8 12 21	19	0 23 16	19	2 30 58	49	6 29
July	10 16 19	20	6 27 46	20	7 3 57	20	2 38 55	50	6 37
Aug.	3 17 57	40	1 25 32	21	1 14 39	21	2 46 52	51	6 45
Sept.	8 19 35	60	8 23 19	22	7 25 21	22	2 54 49	52	6 53
Oct.	7 10 31	80	3 21 5	23	2 6 3	23	3 2 45	53	7 1
Nov.	0 12 9	100	10 18 51	24	8 16 45	24	3 10 42	54	7 9
Dec.	11 3 5	120	5 15 27	25	2 27 27	25	3 18 39	55	7 17
In a leap year after <i>February</i> add the motion of one day.				26	9 8 9	26	3 26 35	56	7 25
				27	3 18 50	27	3 34 32	57	7 33
				28	9 29 32	28	3 42 28	58	7 41
				29	4 10 14	29	3 50 25	59	7 49
				30	10 20 56	30	3 58 22	60	7 57

A Table of the mean motions of *Saturn's* innermost satellice but one, discover'd by *M. Cassini* in 1686.

Beg. Jul. year.	Epoch	Years	Mean mot.	D.	Mean mot.	H. Sc. °	Mean mot.	Mean mot.
	° ' "		° ' "		° ' "	M. ° ' "	M. ° ' "	° ' "
1681	☿ 3 23	1	4 13 2	1	4 11 32	10 5 29	31 2 50	
1686	☿ 5 25	2	8 20 4	2	8 23 4	20 10 58	32 2 50	
1701	☿ 22 3	3	1 0 6	3	1 4 36	30 16 26	33 3 1	
1714	☿ 7 5	4	9 21 40	4	5 16 8	40 21 55	34 3 7	
1715	☿ 17 7	5	2 1 42	5	9 27 40	50 27 24	35 3 12	
1716	☿ 27 9	6	6 11 44	6	2 9 12	60 32 53	36 3 17	
1717	☿ 18 43	7	10 21 46	7	6 20 44	70 38 22	37 3 23	
1718	☿ 28 45	8	7 13 20	8	11 2 16	80 43 51	38 3 28	
1719	☿ 8 47	9	11 23 22	9	3 13 49	90 49 19	39 3 34	
1720	☿ 18 49	10	4 3 24	10	7 25 21	100 54 48	40 3 40	
mon.	mean mot.	11	8 13 26	11	0 6 53	110 0 17	41 3 45	
com.		12	5 5 0	12	4 18 25	120 5 46	42 3 51	
year.		13	9 15 2	13	8 29 57	130 11 15	43 3 56	
Jan.	0 0 0	14	1 25 4	14	1 11 29	140 16 44	44 4 1	
Feb.	3 27 34	15	6 5 6	15	5 23 1	150 22 13	45 4 7	
Mar.	6 20 32	16	2 26 40	16	10 4 33	160 27 42	46 4 12	
Apr.	10 18 6	17	7 6 42	17	2 16 5	170 33 11	47 4 17	
May	10 4 7	18	11 16 44	18	6 27 37	180 38 39	48 4 23	
June	2 1 41	19	3 26 46	19	11 9 9	190 44 8	49 4 28	
July	1 17 43	20	0 18 20	20	3 20 41	200 49 37	50 4 34	
Aug.	5 15 17	40	1 6 40	21	8 2 13	210 55 6	51 4 39	
Sept.	9 12 51	60	1 25 0	22	0 13 45	220 0 35	52 4 45	
Oct.	8 28 53	80	2 13 20	23	4 25 17	230 6 4	53 4 51	
Nov.	0 26 27	100	3 1 40	24	9 6 49	240 11 32	54 4 56	
Dec.	0 12 28	120	3 20 0	25	1 18 22	250 17 1	55 5 1	
In a leap year after February add the motion of one day.				26	5 29 54	260 22 30	56 5 7	
				27	10 11 26	270 27 59	57 5 12	
				28	2 22 58	280 33 27	58 5 18	
				29	7 4 30	290 38 56	59 5 23	
				30	11 16 2	300 44 25	60 5 29	

A Table of the mean motions of *Saturn's* middle satellite, discovered by *Cassini*; in 1671

Reg. Jul. year.	Epoch. ° ' "	Years.	Mean mot. ° ' "	D.	Mean mot. ° ' "	Mean mot. H. Se. ° ' "	M.	Mean mot. ° ' "
1681	12 16	1	9 17 2	1	2 19 41	10 3 19	31	1 43
1686	27 6	2	7 4 3	2	5 9 23	20 6 38	32	1 46
1701	1 17	3	4 21 5	3	7 29 4	30 9 58	33	1 49
1714	11 43	4	4 27 48	4	10 18 46	40 13 17	34	1 53
1715	28 45	5	2 14 50	5	1 8 27	50 16 36	35	1 56
1716	15 47	6	0 1 52	6	3 28 9	60 19 55	36	2 0
1717	22 30	7	9 18 53	7	6 17 50	70 23 15	37	2 3
1718	9 32	8	9 25 36	8	9 7 31	80 26 34	38	2 6
1719	26 34	9	7 12 38	9	11 27 13	90 29 53	39	2 10
1720	13 35	10	4 29 40	10	2 16 54	100 33 12	40	2 13
mon. com. year	mean mot.	11	2 16 42	11	5 6 36	110 36 31	41	2 16
		12	2 23 25	12	7 26 17	120 39 51	42	2 19
		13	0 10 26	13	0 15 59	130 43 10	43	2 22
Jan	0 0 0	14	9 27 28	14	1 5 40	140 46 29	44	2 26
Feb.	10 10 24	15	7 14 30	15	3 25 21	150 49 48	45	2 29
Mar	0 21 44	16	7 21 13	16	6 15 3	160 53 8	46	2 33
Apr.	11 2 9	17	5 8 15	17	9 4 44	170 56 27	47	2 36
May	6 22 51	18	2 25 16	18	11 24 26	180 59 46	48	2 39
June	5 3 16	19	0 12 18	19	2 14 7	191 3 5	49	2 43
July	0 23 58	20	0 19 1	20	5 3 49	201 6 24	50	2 46
Aug	11 4 23	40	1 8 2	21	7 23 30	211 9 44	51	2 49
Sept.	9 14 47	60	1 27 4	22	0 13 11	221 13 3	52	2 53
Oct.	5 5 30	80	2 16 5	23	1 2 53	231 16 22	53	2 56
Nov.	3 15 54	100	3 5 6	24	3 22 34	241 19 41	54	2 59
Dec.	11 6 27	120	3 24 7	25	6 12 16	251 23 1	55	3 3
In a leap year after <i>February</i> add the motion of one day.				26	9 1 57	261 26 20	56	3 6
				27	11 21 39	271 29 39	57	3 9
				28	2 11 20	281 32 58	58	3 13
				29	5 1 1	291 36 17	59	3 16
				30	7 20 43	301 39 36	60	3 19

A Table of the mean motions of *Saturn's* outermost satellite but one, discovered by *Huygens* in 1655.

Beg. <i>Jul.</i> year.	Epoch.	Years	Mean mot.	D.	Mean mot.	H. M.	M. mo.	M.	Mean motion
1641	W 2 48	1	10 20 35	1	0 22 35	1	0 56	31	29 10
1661	X 13 23	2	9 11 10	2	1 15 9	2	1 53	32	30 6
1681	V 27 58	3	8 1 45	3	2 7 44	3	2 49	33	31 3
1686	M 3 28	4	7 14 55	4	3 0 18	4	3 46	34	31 59
1701	II 12 33	5	6 5 30	5	3 22 53	5	4 42	35	32 55
1714	X 17 53	6	4 26 5	6	4 15 28	6	5 39	36	33 52
1715	W 8 28	7	3 16 40	7	5 8 2	7	6 35	37	34 48
1716	Z 29 3	8	2 29 50	8	6 0 37	8	7 31	38	35 45
1717	Z 12 13	9	1 20 25	9	6 23 12	9	8 28	39	36 41
1718	M 2 48	10	0 11 0	10	7 15 46	10	9 24	40	37 38
1719	W 23 23	11	11 1 35	11	8 8 21	11	10 21	41	38 34
1720	R 13 58	12	10 14 45	12	9 0 55	12	11 17	42	39 31
1721	W 27 8	13	9 5 20	13	9 23 30	13	12 14	43	40 27
mon. com. year	mean mot.	14	7 25 55	14	10 16 5	14	13 10	44	41 24
	s o	15	6 16 30	15	11 8 39	15	14 7	45	42 20
Jan.	0 0 0	16	5 29 40	16	0 1 14	16	15 3	46	43 17
Febr.	11 9 54	17	4 20 15	17	0 22 48	17	16 0	47	44 13
Mar.	8 12 2	18	3 10 50	18	1 16 23	18	16 56	48	45 10
April	7 21 55	19	2 1 25	19	2 8 58	19	17 52	49	46 6
May	6 9 14	20	1 14 35	20	3 1 32	20	18 49	50	47 3
June	5 19 7	40	2 29 10	21	3 24 7	21	19 45	51	47 59
July	4 6 26	60	4 13 45	22	4 16 42	22	20 42	52	48 56
Aug.	3 16 18	80	5 28 20	23	5 19 16	23	21 38	53	49 52
Sep.	2 26 12	100	7 12 55	24	6 1 51	24	22 35	54	50 49
Oct.	1 13 30	120	8 27 30	25	6 24 25	25	23 31	55	51 45
Nov.	0 23 24	140	10 12 5	26	7 17 0	26	24 27	56	52 42
Dec.	11 10 42	160	11 26 40	27	8 9 35	27	25 24	57	53 38
				28	9 2 9	28	26 20	48	54 35
				29	9 24 44	29	27 17	59	55 31
				30	10 17 18	30	28 13	60	56 27
In a leap year after February				29					
add the motion of one day.				30					

A Table of the mean motion of *Saturn's* outermost satellite,
discover'd by *Cassini* in 1671.

Beg. Jul. year.	Epochn. ° ' "	Year.	Mean mot. ° ' "	D.	Mean mot. ° ' "	H. M	M. mo. ° ' "	M. mot.	Mean mot.
1681	V 8 40	1	7 6 32	1	0 4 32	1	0 11 31	5 51	
1686	V 15 50	2	2 13 3	2	0 9 5	2	0 23 32	6 3	
1701	X 11 53	3	9 19 35	3	0 13 37	3	0 34 33	6 14	
1714	X 20 20	4	5 0 39	4	0 18 9	4	0 45 34	6 25	
1715	= 26 52	5	0 7 10	5	0 22 42	5	0 57 35	6 37	
1716	II 3 23	6	7 13 42	6	0 27 14	6	1 8 36	6 48	
1717	V 14 27	7	2 20 13	7	1 1 46	7	1 19 37	7 0	
1718	Q 20 58	8	10 1 17	8	1 6 18	8	1 31 38	7 11	
1719	X 27 30	9	5 7 49	9	1 10 51	9	1 42 39	7 22	
1720	M 4 2	10	0 14 20	10	1 15 23	10	1 53 40	7 34	
mon. com. year	Mean mot. ° ' "	11	7 20 52	11	1 19 55	11	2 5 41	7 45	
		12	3 1 56	12	1 24 28	12	2 16 42	7 56	
		13	10 8 27	13	1 29 0	13	2 27 43	8 8	
Jan.	0 0 0	14	5 14 59	14	2 3 32	14	2 39 44	8 19	
Feb.	4 20 41	15	0 14 20	25	2 8 5	15	2 50 45	8 30	
Mar.	8 27 46	16	8 2 34	16	2 12 37	16	3 1 46	8 42	
Apr.	1 18 27	17	3 9 6	17	2 17 9	17	3 13 47	8 53	
May	6 4 37	18	10 15 37	18	2 21 42	18	3 24 48	9 4	
June	10 25 18	19	5 22 9	19	2 26 14	19	3 35 49	9 16	
July	3 11 27	20	1 3 13	20	3 0 46	20	3 47 50	9 27	
Aug.	8 2 9	40	2 6 26	21	3 5 18	21	3 58 51	9 38	
Sep.	0 22 50	60	3 9 38	22	3 9 5	22	4 9 52	9 50	
Oct.	5 8 59	80	4 12 51	23	3 14 23	23	4 21 53	10 1	
Nov.	9 29 41	100	5 16 4	24	3 18 55	24	4 32 54	10 12	
Dec.	2 15 50	120	6 19 17	25	3 23 28	25	4 43 55	10 24	
In a leap year after February add the motion of one day.				26	3 28 0	26	4 55 56	10 35	
				27	4 2 32	27	5 6 57	10 46	
				28	4 7 5	28	5 17 58	10 58	
				29	4 11 37	29	5 29 59	11 9	
				30	4 16 9	30	5 40 60	11 21	

The mean motions of the satellites being thus constituted, their revolutions come out nearly true, as follows.

	d.	h.	m.	s.
The first or innermost satellite	1	21	18	26 $\frac{1}{2}$
The second or innermost but one	2	17	41	10 $\frac{1}{2}$
The third or middle	4	12	25	10
The fourth or <i>Huygenian</i> satellite	15	22	41	28
The fifth or outermost	79	7	46	00

And supposing, according to that universal law of nature (at least in this our solar system) and which obtains both in the motions of *Jupiter's* satellites and of the moon, as also in the primary planets round the sun; viz. that the centripetal forces towards *Saturn's* centre, are reciprocally in the duplicate ratio of the distances; and consequently, that the cubes of the distances from his center are as the squares of the periodical times; from the given distance and period of the *Huygenian* satellite, the distances of the rest are, as follows.

	Semid. of the ring	Semid. of Sat. body
Distance of the first satellite	1 9289	4 9400
The second	2 4708	5 5593
The third	3 4508	7 7643
The fourth	8 0000	18 0000
The fifth	23 3146	32 4578

All which distances agree pretty well with *M. Cassini's* observations; for, the four innermost satellites describe their orbits in the plane of *Saturn's* ring nearly; that is, in a plane, as to sense parallel to the plane of our equator, whatever some affirm to the contrary; but *M. Cassini* the younger has lately discovered that the fifth satellite describes its orbit in a situation somewhat different from the rest, as may be seen in the *Memoirs of the Royal Academy of Sciences* for the year 1714.

An Account of the Situation of the ancient City of Anderida and other Antiquities in Suffex; by Dr. Tabor. Phil. Trans. N^o 356. p. 783.

WHERE *Tacitus* speaks of *Britain* and its affairs, his descriptions are so lively, that one would think he himself had been there, with *Agricola* his father-in-law; and the expression

tion he makes use of (where he mentions the *Irish Prince Tacit.* in *Vita Agric. cap. 24.*) seems to confirm such a supposition.

The gaining the southern part of this island, was the greatest, if not the only acquisition, made to the Roman empire from the death of *Tiberius* to the sixth year of *Claudius*, which we may well suppose was not passed over in silence, by that excellent historian *Tacitus*; But his four books of annals, that contained the transactions of those nine years, we have reason enough to believe, are irretrievably lost: From the mention *Suetonius* makes of *Claudius's* expedition hither, *Suet. Claud. cap. 7.* it is commonly insinuated, that his conquest in *Britain* cost no blood: Our countryman *Bede*, we may see, was of that opinion, because, in his account of *Claudius*, *Bed. Ecclesi. hist. Gent. Angl. lib. 1. cap. 3.* he copies *Sueton's* words: But *Dio Cassius*, from whom we have the most particular account of that war, gives a quite different relation of the matter; he takes notice of at least four battles, fought with the *Britons* (before *Claudius* came over) by *Aulus Plautius*, who had *Flavius Vespasian*, *Flavius Sabinus* and *Hosidius Geta*, that commanded under him: In the first conflict, *Cataraetacus* was defeated; in the second, *Togodumnus*, and as may be inferred from *Dio's* words, was afterwards slain: From his manner of delivering the story, all these battles seem to have been fought to the south of the river *Thames*, and north of the *Sylva Anderida*, excepting the last; and that in the first campaign, the conquests of *Plautius* could not have extended beyond *Kent* and *Surry*; for, it is likely, *Dio Cassius Hist. Rom. lib. 60 Claud. and p. 768.* that the two first actions happened about the skirts of the *Sylva Anderida*, eastwards of the river *Medway*; and the third which held two days, on the banks of that river; because the *Britons* retiring from the river, where they were routed two days successively, assembled their forces again before their fourth overthrow, in that part of *Kent* that borders on the *Thames*, not far from where it falls into the sea; and having crossed the river, were followed by *Plautius's* *Germans*, and put to flight on that side, which was the fourth action mentioned by *Dio*: *Claudius*, being sent for, comes the second year with powerful succours to *Plautius's* assistance, who with his forces waited his arrival near the *Thames*, probably where he quartered in the winter, which, perhaps, was in that large strong camp, still to be seen not far from *Bromley* in *Kent*, on the river *Ravensbourne*, *Camden Britan. edit. 1695 Col. 213, c.* The emperor joining him, *Dio. Cass. Hist. Rom. lib. 60. p. 679.* immediately crossed the *Thames*; overthrew the *Britons*, posted on the

the other side to oppose him; advanced to *Cynobelin's* chief residence, *Camalodunum*, and took it; then after receiving the homage of some states, he returned to *Rome*.

Considering therefore, that *Claudius* staid but 16 days in this island, *ibid. p. 680.* we must conclude, that he made great dispatch; and that he could only make his progress thro' *Kent, Essex, Hertfordshire, Middlesex and Surry*: As to what besides relates to the *British* war in the time of *Claudius*, we have no other account from *Dio*, than that three years after, *Titus* rescu'd his Father *Vespasian*, when in great danger; but where *Suetonius* treats of *Vespasian's* life in *cap. 4.* we are told, that, when that emperor commanded in *Britain* for *Claudius*, he fought 30 battles, subdued two of the most powerful nations, won 20 towns, and reduced the *Isle of Wight*; of which actions, besides what might have been said in the lost books of the annals, *Tacitus* in other pieces of his, as in *Vita Agricola. cap. 13.* largely hints, that in the reign of *Claudius*, *Vespasian's* behaviour and success in this island, shewed to the world his conduct and courage in military affairs: The same is also taken notice of by *Dio Cass.* in *Hist. Rom. lib. 63. p. 736.* And his conquest of the *Isle of Wight* may imply, that the scene of his actions in *Britain* was in those countries bordering on the south channel rather than in the north; since therefore from the climate, the soil and the more ready conveniencies for foreign trade and correspondence, this part of the country might probably sustain as numerous, as stout and as experienced a people as any other (because *Cesar* takes notice, *de bello Gall. lib. 3.* that they not only lent aid to the *Veneti* in their revolt, but used to assist the *Gauls* in most of their wars against the *Romans* *ibid. lib. 4.*) and since no historian afterwards mentions any disturbance given the *Romans* from the southern parts, we may conclude, *Vespasian* entirely subdued them; and that before he left the island, the methods he established for securing peace, were noways inferior to those he had shewn in making war.

The *Romans* well knew, that those who were strangers to civility could not, without great difficulty, be kept in obedience; as soon, therefore, as the countries they had conquer'd were reduced to some degree of quiet, they endeavour'd to make the people in love with their government, by introducing their arts and customs among them; from that inconsiderable instance recorded by *Pliny lib. 13. cap. 25.* we may see, how ready the *Romans* were to oblige the people under their power with any curiosity that might entertain their senses, in order to endear them

them to the authority they had over them: He tells us cherries were not known in *Italy* till the 580 year of *Rome*, when *L. Lucullus* first brought them thither from *Pontus*, and that in 120 years they were increas'd so much, that not only several other countries but *Britain* was also supplied with them, which must have been about three years after *Claudius* himself had been there; the usual landing from *Rome* being then in the county of *Kent*, that fruit was, undoubtedly, first planted there; and the soil agreeing well with it, may be the reason, that the best and greatest quantity of it is still to be had there.

Agricola, in the second year of his lieutenancy in *Britain*, when in winter quarters, pursu'd the same maxims which *Tacitus* calls *saluberrima consilia*; and as may be inferr'd from an expression of *Cæsar de bello Gall. lib. 1.* at the beginning (where speaking of the *Belgæ* as the bravest people, because utter strangers to the politeness of the *Roman* province) conducive to the same end, to wit, to gain the *Britons* by making them acquainted with the *Roman* manners; he not only in private perswaded them, but he publickly help'd and encourag'd them to build temples, places for public assemblies and private houses after the *Roman* mode; he took care to have the principal youth instructed in the liberal arts; he allur'd them to affect the *Roman* dress; and last of all to engage them the more firmly, gave them a taste of the *Roman* luxury and good fellowship, by introducing the use of shady piazza's and baths, *Tacit. Agric. cap. 21.* and their way of banqueting: But here *Tacitus* may be understood to speak of what was done in order to civilize the northern parts of *Britain*, where *Agricola's* presence was requir'd, the southern parts being, we may suppose, softened and quieted by the same methods near 40 years before, when reduced by *Vespasian*.

From thence it may be inferr'd, that should no other signs of the antiquity of these works be found; yet would the bath denote the age of the pavement, and set it near as high as the most early time, that the *Romans* had any real authority in this island.

As by the loss of some of *Tacitus's Annals*, we may have been depriv'd of the most early history of this country, so likewise for want of ancient religious houses, there has been little or no account left of its Circumstances in the times next after the *Roman* authority expir'd in *Britain*. *Malmsbury, de Gestis Pontific. Angl. Lib. 2.* says, that in his time there were in this the

place only the Abbies of *Battel* and *Lewes*, and those nor long before erected; the earliest mention made of it is by *Bede, Hist. Eccles. lib. 4. c. 13.* who informs us, that Bishop *Wilfrid* in 678 being turn'd out of his diocese of *Northumbria* by king *Ecgfrid*, settled at *Selsey* in 680, and stay'd five years, labouring in the conversion of the neighbouring parts; but without any other account of the county, than the miserable ignorance of the inhabitants and the number of families therein: *Bede* spent most of his time in the monasteries of *Wiremouth* and *Jarrow*, and travell'd little; so that considering the distance from thence to this county, and the different governments and interests that lay between, he may well be excus'd for the few particulars he has left us of it.

The next records are those of *Ethelward*, the *Chronicon Saxonum* and *Henry* arch-deacon of *Huntingdon*: But the more clearly to apprehend the ancient state of this county, consult the best map of it you can procure; and at the west-end you will find *West-harting* and *Stansted*, six or seven miles distant from each other; imagine a streight line drawn from *Harting* to *Bourne* near *Pevensey*, and another drawn from a point, which must be a little south of *Stansted*, to *Brightbelmstone*; what lies north of these lines is the *Weald* or lowlands, formerly the *Sylva Anderida*: what is comprehended between these lines, and bounded by the sea, from *Brightbelmstone* to *Bourne*, is the *Dorons*, so famous for their pleasant situation and fruitfulness; the part south of these lines is a flat champaign ground, terminating like a wedge at *Brightbelmstone*; these two last parts were those only that were inhabited in *Bede's* time; they contain no more than $\frac{1}{4}$ of the whole county; which must be the reason, why *Bede Hist. Eccles. lib. 4. cap. 13.* said, that *Sussex* consisted of no more than 7000 families or farms; whereas, in another place he computes *Kent* to contain 13000.

In the three accounts above mentioned, viz. *Ethelwald Hist. lib. 1 cap. 5. Chronic. Sax. Ann. 477. Hen. Hunting. Hist. lib. 2* it is agreed, that in 477, *Ella*, with his three sons, *Cymen*, *Wlencing* and *Cissa*, landed his forces at *Cymenes-ora* (which, from a charter of king *Cedwallas* to the church of *Selsey*, the learned *Camden* proves to be about *Wittering* near *Selsey*) not far from which he routed the *Britons*, and drove them into the *Weald andredesseige*: Their farther progress is most distinctly and naturally deliver'd by the arch-deacon of *Huntingdon* to the following purpose; 'the Saxons possess'd themselves

of the sea-coast of *Sussex*, spreading gradually over the country till the ninth year after their landing in *Britain*; at which time penetrating still more boldly into the country, the kings and rulers of the *Britons* met at *Wettindesburne*, and fought with *Elle* and his sons, and the victory was almost dubious; for, both armies having suffered very much, they parted: And *Elle* sent to his countrymen to request their assistance.

This county having been invaded in the most western part thereof by the *Saxons*; if what they did afterwards was to possess themselves of it, their progress must have been from west to east; and so much *Henry of Huntingdon's* words plainly imply; he says farther, they were eight years about it; which, if we consider the circumstances of the country, will not be surprising, that it should take up so much time, unless their forces had been very considerable, which we have no warrant from any history to suppose; for, the *Weald*, then uncultivated, must have been very difficult to pass, even in the driest summers: The *Downs*, like a wall (with a terrace-walk on the top) have a very steep descent into it, their whole length, excepting, that every 10 miles or thereabouts, they have deep channels thro' them to give passage to the rivers into the sea; therefore, what was then habitable, being thus cantoned out into so many parcels by rivers, nothing could be more difficult to gain than these cantons, if there had been any forces to defend the passes that should have been attempted; the rivers being deep and muddy, and the morasses on each side broad and boggy: Hence we may conceive, that it was no very difficult task for the *Britons* to defend, nor an easy one for the *Saxons* to gain the country; and indeed, the several old camps, still to be seen on the *Downs*, plainly shew, that scarce any part escaped being a scene of war: Mr. *Camden* mentions but two, *Cisbury* and *Chenbury*; in the new edition of his works Dr. *Harris* has added three more; a *Roman* camp at the *Brile* near *Chichester*, *St. Rook's-hill* and *Gons-hill* near the west limits of the country: Here Dr. *Tabor* gives an account of the rest, in which he first takes notice of those on the north-edge of the *Downs* and that overlook the *Weald*.

First, *Chenbury*, mentioned by Mr. *Camden*, is circular and about two furlongs round, two miles west of *Steyning*, and about three miles north of *Cisbury*; eight miles east from *Chenbury* and over *Poynings*, there is a very large camp, of an oval figure, no less than a mile about, accessible at one narrow neck only, and that fortified with a deep broad ditch and a very high bank; this camp

camp is called *Poor-man's-wall*, perhaps, from its having been a security to the distressed *Britons*: About three miles east from thence, is *Wolsenbury*, on a hill, projected like a bastion, beyond the rest of the *Downs*; it is nearly of a circular figure; and a little more than a furlong in diameter: Near three miles east of *Wolsenbury* on the highest part of the *Downs* in that quarter, is a camp, nearly square, about 60 roods long, and 50 broad, much like a *Roman* camp; the side next the north is secured by the precipice of the hill, which is both very deep and steep; the other three sides have each their *porta* after the *Roman* manner still very visible; the ditch seems to have been no less than 11 foot broad; but the ground having been plough'd, the bank is but low; this is called *Ditchling*, as is the old town under it: Near seven miles farther east, and a mile and a half east of *Lewes*, is the last camp on the north edge of the *Downs*; it goes by the name of *Caburn*, which, perhaps, is but a corruption of the *British* word *Cadir*; the parish below it still retains its *British* name *Glynd*; this is a round camp, scarce three furlongs in circuit; its ditch is very broad and deep, and the rampart within is very high; the places where the tents were pitched are still visible; which, from the strength of the out-works, plainly shews, that those within held it a considerable time; near a quarter of a mile west of it, there is a strong work much larger, but not so perfect; yet enough to shew, that it was made to secure a power, that might lie there to be a check upon those in the strong camp, and prevent their making excursions towards *Lewes*.

The camps on the southern limits of the *Downs* are *St. Rooks* near *Chichester*; *High-down* a small square four miles east of *Arundell*, and in the parish of *Goring*; *Cisbury* four miles south-west of *Seyning*; *Hollingbury* is the only one in the middle of the *Downs*, two miles north of *Brighthelmstone*, and three miles south of *Ditchling*; it is square; the *Porta* still remain; and it contains about five acres: A mile east of *Brighthelmstone*, on the top of a hill, half a mile from the sea, is a camp, with a triple ditch and bank; this also is a square, only the corners are round; the outermost trench measures about $\frac{1}{2}$ of a mile: In the parish of *Telscomb*, about five miles east of the last, there are two camps, but both imperfect; the cliff is a south-sence to one, the other is a mile distant from it; their west sides are both finished with very strong works; they were designed for squares, and contain 12 or 15 acres: At *Meeching* or *Newhaven*, on the point of the hill, which overlooks the harbour's mouth from the

west, is a fortification, called the *Castle*; its banks are very high, and its figure nearly a half-oval, containing about six acres; formerly it might be larger, because the cliff, which forms the diameter, moulders away more or less every year, and falls into the sea: Near a mile east of *Seaford* is another fortification, also called the *Castle*, bounded by the cliff on the south; its figure is almost semicircular; the trench and rampart are large, inclosing 12 acres: The last camp is three miles east of *Cukmere-haven*, near a narrow pass coming up from the sea, called *Burling-gap*; it incloses a hill called *Belltout* of a half oval figure; the works are of the same figure, and measure about $\frac{1}{2}$ of a mile; here also the cliff forms the diameter.

Tho' neither history nor tradition have handed down to us any account, when either of these works were made or by whom used (excepting *Cissbury* by *Cissa*) yet from this view we may conceive, that the calamity of war once raged in all these parts; that the ground was disputed inch by inch; that in the attack, as well as defence of it, the pick-ax and spade were as much made use of, as the sword; and lastly, that unless the aggressors were very numerous, eight years was no long time taken up in dispossessing the inhabitants of such a well fortified country.

Some may imagine, that several of these camps were made by the *Danes*; but by what may be observed from the history of those times, that people seemed not to be so formal an enemy, as to prolong war by encampments; their refuge was to their fleets, that always attended them; so that, when likely to be vigorously opposed, they betook themselves to their ships, and suddenly invaded another part, where was less opposition; and what they could not carry with them, they destroyed with fire and sword.

The archdeacon of *Huntingdon*, in the dedication of his annals to *Alexander* Bishop of *Lincoln*, assures him, that he compiled his history from chronicles, preserved in ancient libraries: There is therefore no doubt, when speaking of the *Saxons* here, he had good authority to say, as above quoted, *magis magisque sibi regionis spatia capeffentes*; and that it could have no other meaning than that they carried their conquest from west to east, *in longinquum*, that is, lengthways; had they entirely made themselves masters of the country, it would have been too late; but before they had wholly gained it, the *Britons* assembled against them; the *Saxon chronicle* says *neah, i. e. propè; Eshel-werd junta*; or as *H. Huntingdon* has it *apud Werredeburne*; where there was such a bloody battle, that each side retired, and left the field; the *Saxons* were so greatly diminished, that *Elia* was

was obliged to send for more forces; this action was in the ninth year after *Elia's* first landing in *Britain*, three years before *Hengist's* death. *A. D.* 485; it so weakened *Elia*, that we hear no more of him, till he received his succours from *Germany*, which did not come, according to *H. Huntingdon*, till the first year of the Emperor *Anastasius*, three years after *Hengist's* death, and six years after the bloody battle, viz. *A. D.* 491.

Elia being thus reinforced, moved again, besieged *Anderida* (according to *Huntingdon*, *urbem munitissimam*) and at last forced the place; and because the besieged made so stout a resistance, he had the barbarity of putting all to the sword, and razing the city, which in *Huntingdon's* time remained desolate.

As to the field where the battle was fought; the Saxons extending their power eastwards, the check that was given them, in all probability, must have been where they pushed on their victories; and it being near *Percevedesburne*, this *Bourne* near *Pevensey* may be the place, since it sounds like the latter part of that name (for, there being no west *Bourne* that it relates to, its name may rather be *Esbourne* than *East-bourne*) and likewise that *Anderida*, the Britons last stake and support, was not far from it: It is probable, therefore, that the battle was fought on the *Downs*, between the last mentioned camp at *Burling-gap* and *East-bourne*; for, there are no where on the *Downs*, marks of a greater battle than there; because from the top of that very high cliff (by the inhabitants called *The three Charles*, and by mariners *Beachy-head*) to *Willington-hill*, which is four miles, the ground is full of large *tumuli* or places of burial; and in several parts within that tract, where the position of the ground seems to offer, there are deep trenches and banks, which one would imagine were breast-works made to defend the front of an army; and the *tumuli* on each side of them seem to shew, there was no small struggle, in forcing as well as in defending them.

Mr. Somner, in his *Roman ports and forts in Kent*, p. 106, does not approve of fixing the situation of *Anderida* at *Newenden*, and is inclined to assign some place in *Sussex* for it; but from a modest deference to the opinions of the learned *Camden* and *Selden*, he drops the matter.

But to come to what our more ancient historians say of it, *Henry of Huntingdon* speaks of it to the following purpose; 'and because the Saxons sustained so many losses there, they destroyed the town so as never afterwards to be rebuilt; there is only to be seen a desolate place, the ruins as it were of a very magnificent city.' *Matthew of Westminster* speaks to the same purpose;

purpose; and there *Elia* resided with his three sons, and began to inhabit that country, which to this day is called *Suthsey*, or the country of the *South-saxons*. From the above-quoted passages, it may be supposed, that the ground where that city stood was not quite forgot, in either of these historians days: *Henry of Huntingdon* being the older historian by 200 years (had *Newenden* been the place) his words might have been true, in saying it was desolate; but it is very improbable, that *Matthew of Westminster* should have said so likewise; or at least, not have taken notice of the act of piety and charity of *Sir Tho. Albuger*, who, in his time, had newly erected a monastery at *Newenden* (*Camd. Brit. Kent, Edit. 1695, col. 211*) for the *Carmelites* who came from *Palestine*: But to let that pass, what authority *Mr. Camden* had for saying *ibid.* that *Hengist* sent for *Elia* out of *Germany* to help him to reduce *Anderida*, does not appear.

From the above-mentioned accounts; and others that might be produced, it is plain, that *Hengist* was dead three years before the siege of *Anderida*: In *Hengist's* life time, we find, that for eight years *Elia* had enough to do in *Suffex*; and the blow he had given him the ninth year at *Essexesburne*, obliged him to be quiet the other two years of *Hengist's* life, till his succours, as above-mentioned, came to him from *Germany*; besides, we have not the least hint from any of our historians, that *Anderida* was an eye-sore, either to *Hengist* or his son *Esk* after him; or that *Elia* assisted the *Kentish Saxons*, or they *Elia* in reducing it; therefore, this must be a supposition only of *Mr. Camden's*, in order to confirm the notion of *Anderida's* being at *Newenden*: Taking no notice, then, of that supposition, we may conclude, that *Newenden* is on the *Kentish* side of the *Limen* (for, so the river *Rother* is called in the *Saxon annals Chron. Sax. A. D. 893*, and by *Matthew of Westminster Fl. hist. A. D. 892*; and the mouth of it is called *portus Limeneus* and *Limene* by *Ethelwerd, lib. 3. cap. 3. A. D. 893* and by *Hen. Huntingdon Hist. lib. 3. Alfr. Reg. An. 19.*) and that *Kent* having been subdued by *Hengist* and his *Saxons*, near 40 years before; the town at the mouth of the *Limen*, and the rest, if any up the stream on the *Kentish* side, were also part of their conquest.

Furthermore, after it had cost *Elia* so much time, and, no doubt, pains too, in reducing the plain ground of *Suffex*, it is not likely he should send for more forces out of *Germany*, to carry them 30 miles thro' the difficulties of a large wood (which he must have done if *Newenden* were the place) to besiege a city, so far from his own, and within the *Kentish-Saxon* limits, especially,

especially, if there be any regard to be had to the words of *Matthew Westminster* above-cited; who, after relating the sad fate of the inhabitants and city of *Anderida*, immediately subjoins, 'Ella and his sons resided there (i. e. in that part of *Sussex* where *Anderida* was) and began to cultivate and improve the country.'

In the last place, from the use the Romans made of *Anderida*, it is not unlikely (as Mr. *Somner*, in his *Roman ports and forts* p. 103. judiciously hints) its place was at *Newenden*; for, being one of the stations, under the *Præfectus littoris Saxonici*, where forces were quartered, to have a watchful eye on the sea, whenever the *Saxon* pyrates came to infest the coast, we may suppose it, like the rest of the garrisons under that officer, conveniently situated for the same purpose; as were *Branodunum*, i. e. *Brancaſter*, *Not. Imperii a Pangirol. cap. 73. p. 161.* at the north point of *Northfolk*; *Gariannonum*, *North-Yarmouth*, or very near it; *Othona*, *Isbancheſter* in *Dengby* hundred in *Essex*, some ages ago swallowed up by the sea; *Regulbium*, *Reculver* in *Kent*; *Rutupis*, *Richborow*; *Dubris*, *Dover*; *Lemannis* (which we must look for from *Chron. Sax. A. D. 893*, four miles east of *Appledore*) probably, *New Romney*, all situated near the sea, and on ground that had a full prospect of it; whereas *Newenden* lies low, at least, eight miles within *Appledore*, on a turning of the river, where the land eastwards must have cut off any prospect of the sea: Add to this, that the Romans having a *Numerus*, cohort or batallion of the *Turnacenses*, in garrison at the *Portus Lemannis*, we may suppose, that they were more prudent than to plant another garrison to watch the motions of the *Saxon* rovers, 12 miles up the little river, quite out of sight of sea, where they could be of no service.

To satisfy such as would have the seat of *Anderida* to have been at *Hastings*, let them consult *Hen. Hunting. hist. lib. 6.* whose words are to the following purpose, 'Harald king of the *Angles*, returning the same day with great joy to *Courtme*, while he was at dinner, a messenger brought him word, that *William Duke of Normandy* had possessed himself of the south coasts, and built a castle at *Hastings*; whereby it appears, that *Hastings* was not a desolate place, in the days of the historians who affirm *Anderida* was so: If at *Pevenſey*; that place was so far from having been raz'd by *Ella* that even after the *Norman* conquest it remain'd a strong castle, where *Odo Bishop of Bayon* and his forces sustained a six weeks siege; and for want of provisions were obliged to surrender to *K. William II.* At

this

this time there is so much of *Pevensey* standing, that, perhaps it is the greatest and most entire remains of Roman building, any where to be seen in *Great Britain*.

From the arguments on the foregoing authorities, *Anderida* must have been somewhere in *Sussex*, not in the west, but east part thereof, and not far from the east-end of the *Downs* near the sea: From the bath, pavement, coins and bricks, it is certain, that the *Romans* had once an abode, and not a short one, at this place near *East-bourne*; from the great extent of foundations about the place where these were discover'd, there was a large town or city there; from the common depth of those foundations under the surface of the ground, the buildings they sustained were quite levell'd or raz'd; and from the coals dug up amongst the rubbish, it is plain that part thereof was burnt: All which circumstances agree pretty well with the account given us of *Anderida*.

The situation likewise of a town in this place, gives sufficient reason to suppose, it was a place of importance, whence it had its name; no part hereabouts being any ways so convenient for a secure settlement, or for such use as the *Romans* might have occasion to make of it; We are inform'd by *Cæsar de bel. Gall. lib. 5.* that the maritime parts of *Britain* (speaking of what he himself saw, viz. the south-east) were inhabited by people from *Belgium*; and that they call'd their settlements by the name of the places from whence they came: It was also the opinion of *Tacitus in Vit. Agric. cap. 11.* that those who inhabited next to *Gaul*, came from *Gaul*; and *Bede in Hist. Eccles. Gent. Angl. lib. 1. cap. 1.* says, that it was the tradition in his time, that the southern part of the island was peopled from *Bretagne*: In *Cæsar's Commentaries lib. 3. and 7.* mention is made of the *Andes*, a city and people belonging to it among the *Celte*, inhabiting on the sea-coast: Time varying the names of things, near 200 years after *Cæsar*, *Ptolemy* calls the city *Anderidum*; and near 250 years after him, when the *Notitia Imperii*, now extant, was in use, the *Classis Anderetianorum*, *Pancirob. Comm. in Notis. Imp. cap. 90. p. 179, 180.* is register'd, and the residence of their admiral fix'd at *Paris*: From whence it is to be infer'd, that tho' the capital of the *Andes* might have been *Angers* near the *Loyre*; yet their country had on the north the *British* channel, and on the east the *Seine* for its boundary; the *British* coast about *East-bourne* is the nearest of any to the mouth of the *Seine*; therefore, according to the usage before *Cæsar's* time, the name

name of *Andersida* is readily accounted for: Moreover, this place seems most naturally seated, for giving an appellation to the large wood, adjoining to it; for, as itself is on the shore, so likewise the *Sylva Andersida* at this place came very near the shore, and a large part of it might be seen from the sea before it; it is true, on the sea off of *Romney*, it might be discover'd, but then the distance was great; at all other parts of the coast, the sight of it from sea is hinder'd by hills or high cliffs.

Setting aside the want of a navigable river, the spot of ground, where this old town stood, yields to none in the county for importance and pleasure; for here, like a wedge, ends the firm soil of the *Downs*; nature has shap'd it like an equilateral triangle, having each side half a mile long; towards the sea on the south-side, it is fenced by a low cliff, of 12, 13, and in some places 20 foot high (in which cliff there is still to be seen a strong foundation, that has acute angles, which shews it to have been a fort rather than a dwelling-house) on the north side is a morass with a large rivulet of very good water; between the west sides and the *Downs* lies a small valley, by which advantage there was formerly a harbour, that could contain a small fleet; the banks on each side of it plainly shew, that it was dug by art; but by weeds and gravel from the sea, and by mould annually added, as is observable in valleys, vide *Phil. Trans.* N^o 274. it is now so rais'd, that it is never overflown but at high spring tides, when a strong wind drives the waves into it: This harbour must have been a good security to part of the west side; but what other works there might have been to defend it from the end of the harbour to the morass, cannot be determined, because the intermediate ground has been for several ages in tillage. It is easy to imagine, of what importance a town, fortified at this place, must have been in those days, when the only pass by land from the west to the east end of the county was through it; for, other there could be none, in several miles north, unless the lands in that tract, which are still very ouzy and soft, had been well drain'd.

As the situation render'd this place strong, it is very pleasant withal; for, the ground is high enough for a good prospect of the neighbouring lowlands, and the country towards *Battel*; besides, it has a commanding view over that bay, which is between *Beachy head* and *Hastings*: If the use the *Romans* made of it, was to guard the coast, there was this advantage belonging to it; to wit, that a centinel on the top of *Beachy*,

not two miles from it, might, in a clear day, without turning his body, see the *Ile of Wight*, the hills in *France* near *Bologne* and the *Nefs* in *Kent*; so that from the *Nefs* to *Selsey* it must have been a small sail that could escape his eye.

Remarks on a Fragment of an old Roman Inscription found in the North of England; by Dr. Jurin. Phil. Trans. N^o 356. p. 813.

DR. Jurin transcrib'd no less than 20 Roman inscriptions (several of which were quite new) he found in the remains of the ruins of the famous *Picts wall* built by the Romans to secure themselves against the incursions of the natives of that part of *Britain* they did not care to conquer; among them the following inscription tho' broken, and in great part illegible, suffices to fix the name of one of the ancient nations of *Britain*, that has hitherto been greatly miscall'd; it is to be seen on the said wall, about two miles west from *Lanercroft-Abbey* near the confines of our two northermost counties, and runs thus

CIVITATE CAT

VVILLAVA

ORVM. L. O[S

CDIO

Here it is observable, that the last A of the second line has a mark that follows it, not unlike the last stroke of an N; and if instead of A we put an N, we shall read it CIVITATE CATVVILLAVNORVM, which we cannot doubt to have been the true name of that people, whom *Dio Cassius lib. 60.* calls *Kassallavoi*; and *Ptolemy* in his *Geography lib. 2. cap. 3.* more falsely *Kassovxlavoi*, the first λ having been mistaken for χ : This nation as appears from *Dio*, were more powerful than their neighbours the *Dobuni* (whom he calls *Boduni*) and had, according to *Ptolemy*, *Verolanium* for their capital, which, it is very probable, was the *Cassivellauni Oppidum* of *Cesar*: So that it should seem, *Cassivellaunus* King of these *Catuvellauni*, when *Cesar* invaded *Britain*, either gave his name to his people, or he took theirs; but he was no doubt the most powerful Prince at that time in *Britain*, since by common consent of the rest, he was made general of their united forces, in defence of their country's cause against the Romans.

Obser.

Observations on a Comet seen at Berlin; by M. Kirch. Phil. Trans. N^o 357. p. 820. Translated from the Latin.

M. Kirch, observing the motions of the heavenly bodies on the 18th of *January* 1718, N. S. at $\frac{1}{4}$ an hour after 7 o'clock in the evening, happen'd to spy a comet towards the north; it was next the right of *Bayer's* γ and β in the *Ursa minor*, and appear'd to the naked eye much brighter than β , tho' a remarkable star of the second magnitude; and tho' much paler, yet of a larger diameter, and pretty bright, especially about its center; when seen thro' the telescope, it appear'd like a bright round *nubecula*; but no signs of a tail could be observ'd, nor could the *Nucleus* be distinguished; it proceeded with a very swift motion from 7 to 9 o'clock, having gone over 4° and $\frac{1}{4}$, as is collected from the observations.

On the 19th and 20th of *January* the heavens were overcast; but on the 21st the comet was gone a pretty way from its late place, and was found in *Cassiopeia*, where it form'd a triangle with the stars δ and ϵ , to wit, at $5^h 45'$ it was in $17^{\circ} 34'$ of *Taurus*, and $49^{\circ} 54'$ N. Lat. afterwards at $9^h 15'$ it was seen in $16^{\circ} 38'$ of *Taurus*, and $49^{\circ} 2'$ N. Lat. Moreover, it decreas'd much, and remitted of its velocity, and besides, appearing paler than before, it did not seem to the naked eye to exceed a star of the fourth magnitude, nor did it proceed in its orbit above a degree and a half in four hours and $\frac{1}{4}$: Its diameter by the telescope was $7'$.

Jan. 23, at 4 o'clock in the morning, the comet form'd an *Isosceles* triangle with δ and ϵ of *Cassiopeia*: being distant from each $2^{\circ} 41'$ and $\frac{1}{4}$: This morning it scarce dispatch'd $\frac{1}{4}$ a degree in two hours time; at ten o'clock at night it was seen in a streight line with δ of *Cassiopeia* and ϵ of *Perseus*, and was distant from the former $3^{\circ} 38'$, and from the latter $3^{\circ} 9'$: its diameter was $5'$, and to the naked eye it appeared like a star of the fifth magnitude.

On the 24th of *January* at 6 o'clock in the morning it had not yet applied to ϵ of *Perseus*, but form'd an *Isosceles* triangle with ν and γ of that constellation, and was distant from each not quite 3° and $\frac{1}{4}$.

Thus far the *Nouvelles Littéraires* p. 43, 44, where the observations on the 18th of *January* are wanting, when the comet mov'd very swift and was very near the earth; whence we might with greater certainty form a judgment both of its true and apparent path; for, it is evident that on the 19th of

January it pass'd very near the north pole of the equator; but, for the sake of such as have a mind accurately to examine these observations, the places of the fix'd stars mentioned here, are annex'd, as taken from the *Catal. Britan.* Whence it will likewise appear that some things in this description of the motion of the comet by M. Kirch are faulty.

The places of the fix'd stars in the beginning of the year 1718.

	Long	N. Lat.
In <i>Ursa Minor</i> Bayer's {	Leo 0 18 0	72 58 10
	Leo 17 35 15	71 13 15
In <i>Cassiopeia</i> — Bayer's {	Taur. 14 0 35	46 23 25
	Tau. 20 50 8	47 31 50
	Tau. 11 36 35	45 4 5
In <i>Perseus</i> — Bayer's {	Tau. 8 32 10	55 23 45
	Tau. 10 41 35	56 49 15
	Tau. 12 15 20	56 18 37

An Eclipse of the Sun at Norimberg; by M. Wurtzelbau and at Berlin; by M. Kirch. *Phil. Transf.* N^o 357. p. 812
Translated from the Latin.

IN the *Nov. Literar. Berolin.* we have two observations of a small eclipse of the sun on Feb. 19. 1718, O. S. the one made at Norimberg by M. Wurtzelbau and the other at Berlin by M. Kirch.

At Norimberg the sun rose somewhat eclips'd in his superior limb, which at length became full three digits; the eclipse ended at 8^h 8' 48" about 60° from the vertex to the left.

At Berlin the sun began to be eclips'd, immediately upon his rising, viz. at 6^h 49' or 49' and $\frac{1}{2}$: About the middle of the eclipse, to wit, at 7^h 35' the remaining clear parts in the sun were 24' 40"; consequently, 2 dig. 50' were occluded. The end was at 8^h 28' 20".

Observations on a Roman Inscription found near Lancaster;
by Mr. Gale. Phil. Trans. N^o 337. p. 823.

DR. Hunter having in *Phil. Trans.* N^o 334. only given us his conjectures as to the first fortifying the place where this inscription was found, and the time of its repairs after it had been destroyed, but having said nothing to explain the inscription itself, Mr. Gale offers some thoughts upon it: He does not dispute the time of its foundation, as fixed by the Dr. but begins with the place where it was discovered, namely, *Lancaster*, or *Lancaster* in the Bishoprick of *Durham*, which he is fully persuaded with the Dr. was the *Longovicus*, where the *Notitia Imperii* fol. 176. places the *Numerus Longovicariorum*.

This place was seated upon a large military way, about 12 miles distant from *Binchester*, and seven from *Ebchester*; the one the *Vinovia*, and the other the *Vindomora* of *Antoninus*, as the correspondence of the numbers may evince; *Binchester* being 19 Roman miles from *Ebchester*, as that is 9 from *Corbridge*, the exact numbers the *Itinerary* gives us between *Vinovia*, *Vindomora* and *Corstopitum*; what is very surprising is, that the *Itinerary*, which must go upon the great road directly thro' this town of *Longovicus* between *Vindomora* and *Vinovia*, does not take the least notice of it, but measures the way at the whole length and number of miles from the former to the latter of those stations: If *Longovicus* was founded, as the Dr. supposes, so early as the time of *Julius Agricola*, and if that *Itinerary* was composed by any of the Emperors that bore the name of *Antoninus*, this station might have been destroyed or deserted during the wars with the *Britons*, and not being repaired till the reign of *Gordian III.* was omitted by the author of the *Itinerary*, as a camp not then in being, or of no use to the *Roman* armies; and this would be no weak argument for the antiquity of that work; and perhaps some parts of it may have been described as early as the reigns of those Emperors, or earlier, and such names of more modern places as are found therein, might have been afterwards added, as occasion required: As a farther confirmation of this conjecture, Mr. Gale observes, that this place, after it was repaired by *Gordian*, subsisted even till the ruin of the *Roman* Empire in *Britain*, as is evident by the mention of it in the *Notitia Imperii*; so that had this journey, *Iter. I. a Limite prætor. usque*, which carries us from *Vindomora* to *Vinovia* been composed after the reign of *Gordian*, it would be very hard to account for the omission of this remarkable station and town, as
it

it appears to have been, from this and several other inscriptions found therein.

Mr. Gale takes this opportunity to rectify a mistake in the *Essay towards the recovery of the Roman high-ways thro' Britain*, printed in p. 111, 114 of the 9. Vol. of Mr. Hearn's *Itinerary of Leland*, which having brought *Erming-street* (not *Watling-street*, as Dr. Hunter and the country people call it) a little beyond *Cattarrick* in *Yorkshire*, divides it there into two branches, tracing one of them to *Tinmouth*, and the other to *Carlisle*; but omits the main stem of it, that runs almost directly northwards to *Piercebridge*, so to *Denton*, *Houghton*, *Binchester*, *Lanchester*, *Ebchester*, *Corbridge*, and thro' the heart of *Northumberland* into *Scotland*, about a mile and a half to the west of *Berwick*: It is in several places very entire and fair, especially, between *Corbridge* and *Binchester*, the ridge of it there being for the most part two yards high above the level of the soil, and no less than eight yards broad, and all pav'd with stones, that are as even, as if new laid, as Mr. *Watburton* informed Mr. Gale.

Having thus fixed the seat of *Longovicus*, where the inscription was found, let us consider next what sort of place it was; and upon due enquiry it will appear to have been one of the most ancient and most eminent stations the *Romans* were possessed of in these parts: As to its antiquity, Dr. Hunter has made it probable, that we ought to look for it as high as *Julius Agricola's* commanding under *Domitian* in this island: As to its eminency, the inscription which the Dr. gave account of in *Phil. Trans.* N^o 334, as well as several others found there, are an undeniable evidence of its being a place of great importance: But nothing can put that more out of dispute than the first, which was some years ago transmitted by the same hand, and which Mr. Gale inserts here together with that in N^o 354, and the rather because, little or nothing has ever been said upon it, and that they will give great light to each other.

The stone, on which the first is cut, has been broken in two, whereby some of the letters are defaced; however, it may be very well read, as follows: The letters *P R E* in the fourth line Mr. Gale takes to be a mistake of the workman, having seen several copies where they are so transcribed; that they should be *P E R* is evident from the fifth line of the inscription, which runs thus;

Impe-

*Imperator Caesar Marcus Antonius Gordianus
Pius felix Augustus balneum cum
Basilica a solo instruxit
Per Cneium Lucilianum Legatum Augustalem
Proprætorem curante Marco Aurelio
Quirino præfecto Cohortis primæ Longovitariorum
or rather Legionis Gordianæ*

The other inscription can be read only in the following manner;

*Imperator Caesar Marcus Antonius
Gordianus pius felix Augustus
Principia & Armamentaria
Contulsa restituit
Per Mæcilium Fuscum Legatum
Augustalem proprætorem curante Marco Aurelio
Quirino præfecto Cohortis primæ Legionis Gordianæ*

From these two inscriptions compared together, it will be apparent, that they were not only erected under the same Emperor, but by the care of the very same person *Aurelius Quirinus*, tho' not in the same year: The Emperor can be no other than *Gordianus* the youngest, or third of that name; the two former having been slain so soon after they had assumed the purple, that it is improbable, they should have given any orders or commands for erecting new and repairing old buildings, in so remote a province as *Britain* was from *Africk*, where they were murdered after a short joint reign of scarce seven weeks.

Dr. Hunter in *Phil. Trans.* N° 266. tells us, that the first was dug up about 100 yards east from a large square, which had been fortified with a thick strong wall, faced with hewn stone, on the inside and outside of which, especially towards the east, are nothing but ruinous heaps of stone, and he takes it, that the garrison lodged only within these walls; his conjecture is very much confirmed by the account he gives us of the finding the last inscription within that square inclosure: So that there seems to have been at this *Longovicus* a large town, and one of those camps called *Castra stativa*, where the legions lay in quarters during the time of peace and quiet.

The first inscription tells us, that the Emperor *Gordian* built the *Balneum* and *Basilica a solo*, from the ground; whereas, by the second he appears to have been only the repairer of the *Principia* and *Armamentaria*; wherefore, there might perhaps be

be no town here, till the *Romans* thought fit to repair their old deserted camp at this place; and then the Emperor might also build the bath and palace or the residence of the *Proprator*, when in these parts of *Britain*; the word *Basilica* importing both a palace, and an edifice for hearing of causes and transacting all public affairs: As this eminent building was erected by the Emperor's command, it is an undeniable argument of the splendor of this town, as the large heaps of rubbish and ruins, where this inscription was found, are of its largeness and extent.

The second inscription equally puts the being of the *Castrum stativum* out of dispute, when it acquaints us with the rebuilding of the *Armamentaria* and *Principia* there, that is, the arsenals and quarters either of the legionary soldiers, that were called the *Principes*, or the place where the eagles and other military ensigns were kept: It is probable, they did not belong to one particular legion, but to several, as they had occasion to be employed here; tho' the *Legio sexta Victrix* *Prot. Leg. 6. Niceph. Ebor. locat.* seems to have the best title to them, as being constantly quartered in the north; whereas, the *Legio secunda* *Anton. Itin. 12. Not. Imper. p. 161.* and the *Legio Vicesima* *Anton. Itin. 2.* were generally garrisoned the first at *Caerleon* in *Wales* and *Richburrow* in *Kent*; and the other at and about *Chester*; So that the monuments *Camd. p. 835, 920. Phil. Trans. N° 264.* they have left in the north were erected by them, when the wars and other works, as particularly the walls carried cross the island, called them thither; which being finished they returned to their more southern quarters, and continued therein, till commanded abroad upon new services: Mr. Gale does not pretend to determine, when these *Armamentaria* and *Principia* first fell to ruin; perhaps, it might be when *Hadrian*, *Lollius Urbicus* and *Severus* had carried their conquests farther into the enemy's country, and having built those famous walls, the relicks of which we still see in the shire of *Sterling* in *Scotland*, and in *Northumberland* and *Cumberland* in *England*, that this camp might be thought useless, the *Roman* forces being drawn nearer to and quartered upon the frontiers; and so this fortress was deserted and suffered to fall into decay, as the word *conlapsa* implies; and not that it was destroyed by any fire, war, or other enemy, than age and neglect.

Tho' the word *conlapsa* be wrote here with an N, there can be no doubt, but the pronunciation of it was, as we usually find it spelt, *collapsa*, a certain argument of the letter N being

ing quiescent in the middle of a word before two consonants, especially NS and NT, when the T was pronounced like an S! To omit what *Quintilian Instit. lib. 1. cap. 7.* says to this purpose, it is confirmed by the want of that letter in innumerable inscriptions in *Gruter, Reimfius, &c.* and it is not surprising, since the workmen in those days, as well as ours, usually wrote, as they spoke their words; as a proof of what Mr. Gale says, he affirms from ocular inspection and a most accurate examination, that there is no transverse line over the letters ES in the word TABRIGESIS in the inscription of *Jub. Vitalis at Bath*; whatever has been affirmed to the contrary, but that the letter N is entirely omitted there: As a further proof of the foregoing assertion, Mr. Gale explains a short inscription belonging to some of our countrymen, tho' found at *Amerbach in Germany, Gruter, p. 93.*

NYMPHIS *Nymphis*

N^o BRITTON *Numerus Britannus*

TRIPUTIEN *Tripuntianus* or — *curat*

SUB CURA *Sub cura*

M^o ULPI *Marci Ulpi*

MALCHI *Malibi*

LEG XXII *Centurionis Leg. 22.*

PR^o P^o P^o *Primigenia, pia, Felici.*

There is no difficulty but in the word TRIPUTIEN, and that will presently vanish, if you insert the letter N and read it TRIPUNT, i. e. TRIPONTIENUS or TRIPONTIENSIS, the mutation of the O and V being frequent; this will bring you to *Tripontium* or *Dorobridge* in *Northamptonshire, Antonin. Itin. 6.* tho' Dr. Battley in his *Antiquitates Rutupinae* p. 21. would read it RIPUTIEN, and fix the place whence this *Numerus* took its appellation at *Richburrow in Kent.*

But to return to the camp at *Longovicus*, it will be as difficult to assign a reason for its being repaired, as it was for its being deserted; unless the *Proprators* might judge it advisable about the time of *Gordian III.* to fix their residence there, and consequently, re-fortify the old camp and form their state and security; and that it was not re-fortified upon any sudden emergency, but for duration, is evident both from the strong stone-works that encompassed it, and from a body of forces that lay here, even at the expiration of the *Roman Empire* and authority in this island, which, from its continuance in the

same station, had got the name of the *Longoviciani*, *Not. Imper. fol. 176. b.*

The person who, under the Emperor, directed these repairs was *Mæcilius Fuscus*: As *Mæcilius* is a diminutive of *Mæcius*, it is not unlikely, that he was the son of *Mæcius Fuscus*, who was Consul with *Turpilius Dexter* A. D. 225. in the reign of *Alexander Severus*: By this inscription it appears, that this *Mæcilius* was the Emperor *Gordian's* lieutenant and *Proprætor* in *Britain*; and as the name of *Fuscus* stands in the same place in the one inscription, as that of *Lucilianus* does in the other, and with the same adjuncts both before and after, we may fairly conclude, that he was either his predecessor or successor; but which, is impossible to determine.

And here it may be remark'd, that we never meet with a *Legatus Augustalis* in any inscription in this island, without the joint title of *Proprætor*; and *Tacitus* himself in his *Annals lib. 12. cap. 32.* either makes them the same office, or at least unites them in the same person, when he tells us, in *Britannia* *P. Ostorium Proprætorem turbide res exsepere*; and having presently after given an account of the manner of the fight with the *Iceni*, styles him *Legatus, quâ pugna filius Legati, M. Ostorius servati civis decus meruit, ibid. cap. 39.* and a little after he gives both the same titles to *A. Didius*, *Ostorius's* successor.

From these two monuments we have an account of the Roman arms and magnificence at *Longovicus*, and the names of two *Legates* and *Proprætors* of *Britain*, that would otherways have been buried in oblivion, viz. *Cneius Lucilianus* and *Mæcilius Fuscus*: For, from *Virius Lupus* (who was *Proprætor* and *Legate* in *Britain* about A. D. 208, under *Severus*, and just before that Emperor's coming into this island, repaired a bath burnt down at *Lavatra* or *Bowes* in *Torkshire, Camd. p. 762. Edit. 1695.*) we have no where extant the name of one of those officers, till we come to *Nonnius Philippus*, whom *Mr. Gale* takes to have succeeded the last of these; the stone, as in *Camd. Britan. p. 830.* which was found at *Old Carlisle* in *Cumberland*, and has preserved his memory, setting forth, that he was *Legate* and *Proprætor*, when *Atticus* and *Prætextatus* were Consuls, which was A. D. 242, the very year that our *Gordian* went upon his *Persian* expedition, and from which he never returned: And as that Emperor left *Nonnius Philippus* in that post, when he marched into the east, where he was murdered about two years after, it is highly probable, that he

was the last *Proprætor* of his appointing, and consequently, that *Mæcilius Fuscus* was his predecessor, and that the repairs were begun at *Longovicus* before the year 243. This inscription is wrong transcribed in *Camden*.

I. O. M.

PRO. SALUTE. IMPERATORIS M. ANTONI. GORDIANI.
P. F. INVICTI AVG. ET. SABINE. FVR JE. TRANQUILE.
CONJUGI. EJUS. TOTIQUE. DOMU. DIVIN. EORUM. A
LA. AUG. GORDIA. OB VIRTUTEM APPELLAT. POSUIT.
CUI. PRÆEST AIMILIUS. CRISPINUS. PRÆ EQQ. NATUS.
IN. PRO. AFRICA. DE TUSDRO SUB. CUR. NONNII. PHI-
LIPPI. LEG. AUG. PROPRETO. ATTICO. ET. PRETEXTA-
TO. COSS.

The person, who had the care of these repairs both in the town and camp, was *Marcus Aurelius Quirinus*, *Præfekt* or commander of a company of foot; which is another argument, that the *Proprætors* *Lucilianus* and *Fuscus* immediately succeeded each other, he serving in the same post under both: However Mr. Gale observes, that tho' the two first inscriptions have been cut very near the same time, and by the same hand, as appears by the form of the letters, and manner of the abbreviations in each of them; yet the office that this *Quirinus* bore, is somewhat differently expressed in the first from what it is in the second, if they have been accurately transcribed; the first shewing after *QUIRINO* the letters *P R E. C H. I. L G. R.* which before Mr. Gale had seen the second, he was induced to read *Præfekt Cohortis primæ Longovicariorum*, the *Not. Imper. fol. 226. b.* placing the *præfektus Numeri Longovicariorum* at *Longovicus*; that *Numerus* and *Cohors* were the same thing, *Pancirollus* in his notes *fol. 16. b.* quotes *St. Chrysostom* and some others to prove, *cohors erat qui vocatur numerus*; but Mr. Gale rather takes it to be an indefinite number of men, which might comprize several companies, independent of any legion; *Vegetius lib. 2. cap. 9.* speaking of the *Legati Imperatoris*, says, *in quorum locum nunc illustres viros constat Magistros militum substitutas, in quibus non tantum binæ Legiones sed plures numeri gubernantur*; by which it is plain, the *Numeri* were no legionary cohorts; nor was the name so modern, as from the *Notitia Imper.* and *Chrysostom* it might appear to be; for, we meet with a *Numerus Britonum*, *Gruter p. 942.* upon an altar found in *Transylvania*, dedicated to the nymphs, when the Emperor *Commodus* and *Glabrio* were *Consuls*, A. D. 186; and another

Numerus, *ibid.* 8, 10. upon an altar, erected to Hercules for the prosperity of *Septimius Severus*, when *Latoranus* and *Rufinus* were Consuls A. D. 197. But after reviewing the letters at the end of the second inscription, which are plainly transcribed P. R. C. O. H. I. L. C. O. R. Mr. Gale could read them no other-ways than *Praefectus Cohortis primae Legionis Gordianae*: *Gordian* III. was so beloved of the soldiery, that several legions complimented him by taking his name, as the *Legio tertia Italica*, *Vell. Notum. Auguste Vindob.* p. 421. which took the additional name of *Gordiana*; and the *Legio decima gemina*, *Grut.* p. 80. and *Decima tertia gemina*, *ibid.* p. 421. i. did both assume the same appellation: But which of the legions, quartered in this island, called itself so, is not determined by this inscription, or any other Mr. Gale knows of; however, as the *Legio sexta victrix* was all along quartered in the northern parts of this kingdom, where these inscriptions were made, he doubts not but it was that which called itself *Gordiana*, tho' the numeral distinction of VI. be omitted, only perhaps for want of room on the stone: We find by several inscriptions in *Camden*, that there was an *Ala* in those parts that valued itself on its prowess, and was therefore, called the *Ala Augusta*. Of the several memorials it has left us of its appellation, Mr. Gale mentions one, found at *Old Carlisle*, *Camd.* p. 827. and which is the most ancient of them all by any certainty of date.

AL. AUG. OB
RTUT. APPEL. CUI
PRAEST. TIB. CL. TIB. F. P.
LING. . . N. JUSTINUS.
PRAEF. FUSCIANO
IL. SILANO. IL. COS.

That is, *Jovis optimo maximo Ala Augusta ob virtutem appellata, cui Praef. Tiberius Claudius Tiberii filius, Provincia Lingonensis, Justinus Praefectus, Fusciano secundo, Silano secundo Consulibus.*

This altar was dedicated, when *Fuscianus* and *Silanus* were Consuls the second time, that is, A. D. 188. under the reign of *Commodus*, and 30 years before our *Gordian* came to the Empire: At the same place also was discovered the inscription mentioned before, where we find this same *Ala Augusta* calling itself *Gordiana*; whence Mr. Gale thinks it probable, that the legion to which this *Wing* belonged, was the *Legio Gordiana*

diadema, mentioned in the inscriptions found at *Langobester*, and that that legion was the *Legio sexta victrix*, from the long continuance of this *Alo. Augusta* in these northern parts of the nation, the constant quarters of that legion.

An Account of a fiery Meteor in Jamaica; by Mr. Barham. Phil. Trans. N° 357. P. 837.

ABOUT the year 1700, as Mr. Barham was riding one morning from his habitation about three miles north-west from *St. Jago de la Vega*, he observed a ball of fire, of the bigness of a bomb, swiftly falling down with a great blaze; it fell as he took it into the town; but when he came within a quarter of a mile thereof, he saw there several people gathered together a little to the south in the *Savanna*, where they were wondering at the ground being strangely broken and plough'd in by a ball of fire, which, as they said, fell down there; Mr. Barham observed several holes in the ground: one in the middle as big as a man's head and five or six smaller holes round it, as big as a man's fist, and so deep, especially, the biggest, as not to be fathom'd by long switches or sticks: It was observed, that the green grass near the holes was quite burnt, and, that a strong smell of sulphur remained thereabouts for a good while after.

They had a terrible rainy night before, with a deal of lightning and loud thunder-claps, which are very frequent in *Jamaica*, often killing the cattle in the fields: One Mr. Lord, who lived at *Dry river*, had two sons struck dead with lightning in 1716, without any wounds or appearance of hurt found about them: And as these claps are much louder and stronger than any he had ever heard in *Europe*, so their showers of rain pour down in a most violent manner. They have lightning all the year round; but their great rains are in the months of *May*, *August* and *October*: Mr. Barham has seen *May* for two or three years without rain, which was looked upon as a prodigy; and they paid dear for it in their *Indigo*; for, a caterpillar, which may-rains were wont to destroy, appeared and wove a fine silk about the *Indigo*-plant, and destroyed it quite without hurting any thing else; *August* and *October* never go out without a flood; for, then they have from the sea universal rains all over the island; and they have often rains in the mountains from the clouds that lodge there, when they have none in the low-lands.

The island is full of mines; and if search'd into, Mr. Barham doubts not but that they are very rich: The island is very subject

subject to earthquakes, several happening every year, especially after great rains, which fill up all the large cracks in the surface of the earth; for, in a very dry season, they are so very large, deep, and gaping, that it is dangerous to ride over some parts of the *Savannas*, for fear a horse should get his legs into them. The earthquakes make a noise or rumbling in the earth, before the shock is felt, and they seem to run swiftly to the westward.

An Account of the antiquity of the Venereal Disease, by Mr. Beckett. Phil. Trans. N^o 357. p. 839.

IT has been the general opinion, that the venereal disease was not known at least in Europe, till about the year 1494. Notwithstanding which, Mr. Beckett makes it evident, that it was frequent amongst us some hundreds of years before that date; and that without quoting any ancient authorities from Hippocrates, Galen, Celsus, Avicenna, &c. or from the holy Scriptures; as has been sufficiently done already by several, who have wrote of the antiquity of this disease, and particularly, by Dr. Pating, but by only consulting our own antiquities and the ancient English authors in physic and surgery, we may be furnished with instances of the frequency of this distemper among us, long before even our modern authors imagined it made its appearance in Europe.

As to the first degree of this disease, Mr. Beckett proves from authentic vouchers that it was anciently called the *Stinking* or *Burning*; and that this term was successively continued for several hundreds of years, to signify the same disease we now call a *clap*, till that latter appellation first took its rise. And as the most likely method of accomplishing his design, Mr. Beckett first examines into those records, that relate to the *stews*, allowed by authority to be kept on the bank-side in *Southwark*, under the jurisdiction of the bishop of *Winchester*, and suppressed the 12th of *Henry VIII.* for, it is impossible, but, if there were any such distemper in being at that time, it must be pretty common among those leud women, who had a licence for entertaining their paramours, notwithstanding any rules or orders, which might be established to prevent its increase. But if we shall find, that there were orders established to prevent the spreading of such a disease, that persons might be secure from any contagious malady after their entertainment at those houses (which were formerly 18 in number, but in the reign of *Henry VII.* reduced to 12) we may then securely conclude, that it was the frequency of the disease that put those, who had the authority, under a necessity

necessity of making such rules and orders; for the same powers that granted a liberty for keeping open such leud houses, must find it their interest to secure, as much as possible, all persons from receiving any injury there: lest the frequency of such misfortunes should deter others from frequenting them, and consequently the original design of their institution cease, from the entire sinking of the revenues: Now Mr. Beckett finds, that, as early as the year 1162, divers constitutions relating to the Lordship of Winchester (being also confirm'd by the King) were to be kept for ever, according to the old customs that had been time out of mind: among which were the following, viz. No *Stew-holder* to take more for a woman's chamber in the week than 14 pence; not to keep open his doors upon holy days: No single woman to be kept against her will, that would leave her sin: No single woman to take money to lie with any man, unless she lie with him all night till the morning: No *Stew-holder* to keep any woman, that hath the perilous infirmity of Burning.

These and several other rules were to be strictly observ'd, or the offenders to be severely punish'd: Now we are assur'd, that no other disease can be communicated by carnal conversation with women, but the venereal, because, that alone is contagious; and it is evident, the Burning was certainly so; for, had it been nothing other than some simple ulceration, heat or inflammation there would have been no contagion; and that affecting the woman only, could not be communicated by any venereal congress, and consequently, would not infer a necessity of her being comprehended under the restraining article: These orders likewise prove the disease much more ancient than the above-mentioned date; because they were only a renewal of such as had been before established time out of mind.

But to confirm this farther, Mr. Beckett finds, that in the custody of the Bishop of Winchester, whose palace was situated on the bank-side, near the *Stews*, was a book written on Vellum, the title of which runs thus, *Here begonne the ordinances, rules and customes, as well for the salvation of mannes life, as for to aschewe many myschiefs and inconveniencs that dayley be lik there for to fall owte, to be rightfully kept, and due execution of them to be don unto any personne within the saur.* One of the articles begins thus; *de his qui custodiunt mulieres habentes nephandam infirmitatem*; it goes on, *Item, That no Stew-holder keep noo woman wythin his hous that hath any sicknesse of veneryas, but that he be putte out upon the pene of make it a fene into the Lord of a hundred shylings.*

This

This is taken from the original manuscript, which was preserv'd in the Bishop's court, suppos'd to be written about the year 1430. From these orders we may observe the frequency of the distemper at that time, which, with other inconveniencies, was daily lit there for to fall short; and the greatness of the penalty (as the value of money then was) laid thereon, proves it to be no trifling or insignificant matter.

But the bare proof of their having been anciently such a disease, as was call'd the Burning, may be thought insufficient, unless we were perfectly assur'd what it was, and how it was describ'd in those times; and this Mr. Beckett does from an unquestionable authority, which is that of Mr. Arden, who was one of the surgeons of our king Richard II. and likewise to king Henry IV: In a curious manuscript of his on vellum, he defines it to be a certain inward heat and excoriation of the *Urethra*: which description gives us a perfect idea of what we now call a Clap; for, frequent dissections of such, as labour'd under that disease, have made it evident, that their *Urethra* is excoriated by the virulency of the matter they receive from the infected woman; and this excoriation or ulceration is not confin'd to the *Ostiola* or mouths of the *glandulae mucosae*, as has been lately thought, but may equally affect any part of the *Urethra*, that is not out of the reach of the impell'd malignant matter: The heat above describ'd, which such patients are sensible of, as well now as formerly, is a consequence of the excoriated *Urethra*; for the salts contained in the urine must necessarily prick and irritate the nervous fibrils, and excite a heat in those parts of the *Urethra*, divested of their natural membrane; which heat will always be observ'd to be more or less, as the salts are diluted with a greater or less quantity of urine; a thing Mr. Beckett has often observ'd in persons that have labour'd under this infirmity in hot weather, when the perspirable matter being thrown off in greater quantities, the salts bear a greater proportion to the quantity of the urine, and thereby make its discharge at that time so much the more painful and troublesome.

Thus we see a very early and plain description of this disease among us to be entirely conformable to the latest and most exact anatomical discoveries; here is no tone of the testicles deprav'd, according to *Trajanus Petronius*; no exulceration of the *Parastate*, according to *Rondeletius*; no ulceration of the seminal vessels, according to *Platerus*; no seat of the disease in the *vesicula seminales* or *Prostate*, according to *Bartolin*;

nor in those parts, and the *restes* at the same time, according to *Wharion* and others, who have erroneously fix'd the seat of this disease, and whose notions in this respect are now justly exploded; but a simple and true description of it, and its situation about 150 years before any of those gentlemen oblig'd the world with their labours.

Next, as to the ancient method made use of to cure the Burning; we are not to expect the measures our predecessors, in those early times, made use of, should be calculated for the removing any malignity in the mass of blood, or other juices, according to the practice in venereal cases at this time; because they look'd on the disease to be entirely local, and the whole of the cure to depend upon the removal of the symptoms: Hence it was they recommended such remedies as were accommodated to the taking off the inward heat of the part, and cure the excoriations or ulcerations of the *Urethra*. The process for accomplishing this *Mr. Beckett* sets down from the above-mentioned *Mr. Arden*, who wrote about the year 1380; his words are as follows, *Item contra Incanvium virge virilis interior ex calore & excoriations fiat talis syringa (i. e. injectio) lentior: Accipe lac mulieris masculam nutrientis & parum zucarum, oleum violæ & pistane, quibus commixtis per syringam infundatur, & si prædictis admiscueris lac amigdalorum melior erat medicina.* There is no doubt but this remedy, made use of to patients at this time, would infallibly take off the inward heat of the part, and cure the excoriations or ulcerations of the *Urethra*, by which means what issued from thence would be entirely stopp'd; and this was all they expected from their medicines, in as much as they were entirely unacquainted with the nature of the distemper; and did not in the least imagine, but if the symptoms that first attack'd the part were removed, the patient was entirely cur'd.

Mr. Beckett, as a farther confirmation of what he has advanced, proceeds to prove, that by this Burning or Burning is meant the venereal disease; which he does by shewing, that succeeding historians, physical and chirurgical authors and others have all along with us in *England* us'd the very same term to signify the venereal distemper: In an old manuscript he had by him, written about the year 1390, there is a receipt for the Burning of the pentill, *gat men clepe ye apegealle; galle* being an old *English* word for a running sore: They who know the etymology of the word *Apron*, cannot be ignorant of this; and in another manuscript, written about 50 years after, there is a receipt for Burning in that part by a woman: *Simon Fish*, a zealous promoter of the reformation in the reign of *Henry VIII.*

in his supplication of beggars, presented to the King in 1530, says as follows; 'these be they (speaking of the *Romish* priests) 'that corrupt the whole generation of mankind in your realm, 'that catch the pockes of one woman, and bear them to another; that be burnt with one woman, and bear it to another.'

But to make this matter still more evident, Mr. *Beckett* observes, that *Andrew Boorde* a Dr. of physick, and a *Romish* priest, in the reign of *Henry VIII.* in a book he wrote, entitled, *The Breviary of health*, printed in 1546, speaks very particularly of this sort of burning; one of his chapters begins thus, The 19. chapter doth shew of burnings of an harlotte, where his notion of communicating the burning is very particular: The same author adds, that if a man be burnt with an harlot, and meddle with another woman within a day, he shall burn the woman that he shall meddle withal; and as an immediate remedy against the burning, he recommends the washing the *Pudenda* two or three times with white-wine, or else with sack and water; but if the matter have continued long, to go to an expert surgeon for help. In his 82. chapter, he speaks of two sorts of burning; the one by fire, and the other by a woman thro' carnal copulation, and infers the person that is burnt of an harlot to another chapter of his for advice; if he get a dorrer or two, so call'd from its protuberance or bunching out; for, about that time the word *Bubo* was mostly made use of, to signify that sort of swelling usually happening in pestilential diseases.

Hence it appears, that the burning, by its consequences, was venereal, since every day's experience makes it evident, that the treating the first symptoms of the disease either by astringent medicines, or removing them by cooling and healing the excoriated parts, will generally be attended with such swellings in the groin, which we rarely observe to happen from any other cause whatsoever.

Mr. *Beckett* gives some more instances of the burning: In a manuscript he had of the vocation of *John Bale* to the Bishopric of *Ossory* in *Ireland*; written by himself, he speaks of Dr. *Hugh Weston* (who was dean of *Windsor* in 1556, but depriv'd by cardinal *Pole* for adultery) as follows, 'at this day is 'lecherous *Weston*, who is more practis'd in the art of 'breth-burning, than all the whores of the *Strews*', and again, speaking of the same person, he says, 'he not long ago brent a 'beggar in *St. Botolph's* parish'; the same author says of him elsewhere,

elsewhere, ' he had been fore bitten with a *Winchester* goose, ' and was not yet healed thereof'; which was a common phrase for the pox at that time, because the *Stews* were under the jurisdiction of the Bishop of *Winchester*. *Mich. Wood* in his epistle before *Steph. Gardiner's oration de verâ obedientiâ*, printed at *Rboan* in 1533, gives another instance of the Burning: And *William Bullein* a physician in the reign of *Queen Elizabeth*, in a book he publish'd, call'd the *Bulwark of Defence*, &c. printed in 1562, bringing in sickness, demanding of health, what he should do with a disease, call'd the *French pockes*, health answers, ' he would not that any should fish for this disease, ' or to be bold when he is bitten to thynke thereby to be ' helped, but rather to eschewe the cause of this infirmity, ' and filthy rotten Burning of harlots."

From these instances it sufficiently appears, that the first degree of the venereal disease was anciently known among us under the title of Burning; and *Mr. Beckett* affirms he could make it appear from his collections, that the disease, when it came to be confirm'd, was no novelty here in thole early times.

Astronomical Observations for the Years 1717, 1718. Phil. Trans. N° 357. p. 840. Translated from the Latin.

THE following observations (which we have collected into one body) are very accurate, as having been made with very long telescopes and the exactest micrometers.

On *April 15th*, 1717, 9h. 49' equated time, *Mr. Pound* observ'd at *Wansted*, that *Jupiter* was return'd to that star, which on the 22. of *November 1716* in the morning he had cover'd with his body, as in *Phil. Trans. N° 350*. And *Jupiter's* center was distant at that time from that star (which is the third of *Gemini* in *Catal. Britan.*) 23' 22" towards the north, and at the same time distant from another neighbouring star (which is the fourth of *Gemini* in the said *Catal.*) 27' 11"; and with this the planet was almost in conjunction.

On the 29th of *April*, 10h. 3' equated time, at *Wansted* *Mr. Pound* observ'd *Jupiter* near four small stars, and preceeding them all, and in the very beginning of *Cancer*, as it were; and the center of the planet was distant from *e* 13'; from *b* 11' 32"; from *f* 19' 53", and from *g* 9' 27".

On the 26th of *April*, 9h. 7' *Jupiter's* center was distant from *e* 8' 35"; from *f* 9'; from *g* 4' 5", and from *b* 13' 50";

and now he had pass'd all but *f* to which he tended, and which he should leave the next day a very little below him.

Almost in the same moment, to wit, at 9 o'clock the star *g* was seen at London in the vertex of the *Isosceles* and almost equilateral triangle with the centre and third satellite of *Jupiter*, then distant six diameters of *Jupiter* to the west, only that the legs were a very little longer than the base; and within a quarter of an hour, the angle at *Jupiter's* centre, which at first was greater than that at the satellite, became sensibly less.

The three stars *b*, *g*, *e* are the 10th, 11th and 12th of *Gemini* in *Catal. Britan.* according to which their situation at that time was as follows; *b* in $22^{\circ} 55''$ of *Cancer*, with $11^{\circ} 25''$ N. Lat. *g* in $28^{\circ} 25''$ of *Cancer*, with $3^{\circ} 40''$ N. Lat. *e* in $29^{\circ} 20''$ of *Cancer*, with $8^{\circ} 5''$ S. Lat. and the fourth star *f* was distant from *g* $11^{\circ} 40''$, from *e* $12^{\circ} 50''$, and lastly from *b* $20^{\circ} 36''$, whence its place will be determined: From which it is manifest, that *Jupiter* had but very little N. Lat. and not exceeding half a minute, at least if we are to have any regard to the above places of the stars; these things may be of use to posterity in determining the motion of *Jupiter's* nodes, if they have any.

On the 18th of *June* the same year at 10h. at London, *Saturn* was seen from the *Society's* house in *Crane-court* very near a telescopic star, from which it was scarce one diameter of his ring distant to the south; and a perpendicular let fall from the star upon the line of the *ansa* met the middle of the eastern *ansa*; this small star, which is in no catalogue, was at that time in $12^{\circ} 58'$ and $\frac{1}{2}$ of *Libra* with $2^{\circ} 33'$ N. Lat. nearly: it has a star accompanying it of equal brightness, $4'$ distant to the east, and somewhat more southerly, whence it may be easily distinguished, and its place verified by any one that has a mind to it.

At 10h. 30' the same night *Mars* was seen near the star that preceeds the 35 of *Scorpio*, from which it was distant $7^{\circ} 16''$ as measur'd by a 24 foot telescope; and that in a streight line produced thro' the bright star θ in the foot of *Ophiuchus* and the said star; this star preceeds the 35 of *Scorpio* $30^{\circ} 27''$ R. *Ascens.* and is more southerly than it $2^{\circ} 28''$, whence its place at that time was in $15^{\circ} 24' 20''$ of *Sagittarius* and $3^{\circ} 59' 25''$ S. Lat. but θ of *Ophiucus* was then in $17^{\circ} 28'$ of *Sagittarius* and $1^{\circ} 47' 38''$ S. Lat. therefore, *Mars* preceeded the star $4^{\circ} 58''$ Long. and was more southerly than it $5^{\circ} 30''$.

After-

Afterwards on the 13. of *September* at 8^h 30' equated time *Mars* was seen by Mr. *Pound* to preceed the bright star σ in *Sagittarius's* shoulder 11' 54": At 8^h 25' the planet's distance from the star was accurately 25'.

On the 5. of *December* 18^h 30' equated time, by the coincidence of several observations, Mr. *Pound* found, that *Saturn* preceeded the bright telescopic star near him 27' 19" *R. Ascen.* and was more southerly than the said star 1' 59"; at the same time *Saturn* preceeded α in *Virgo's* train 1° 25' 21" and was more southerly than it 4' 5"; hence *Saturn's* place was in 29° 16' 21" of *Libra*, with 2° 22' 21" N. Lat. The telescopic star was at that time in 29° 40' 56" of *Libra* and 2° 33' 43" N. Lat.

January 7. 1718 at 5^h 30' equated time, *Venus* was observed near two fixed stars, omitted in *Catal. Britan.* she was more northerly than either, being distant from the preceeding 32' 30" and from the following 17' 30"; the preceeding star was then in 14° 42' 20" of *Pisces* with 40' 10" S. Lat. and the following was in 15° 21' 55" of *Pisces*, with 27' 15" S. Lat. as may be gathered from Mr. *Flamsteed's* observations.

January 18. 8^h equated time, *Jupiter* preceeded π in *Cancer's* breast 3° 30' 50" *R. Ascen.* and was more southerly than the star 14' 15"; hence *Jupiter's* place is 28° 20" with 36' 45" N. Lat.

March 11. 10^h 36' equated time *Saturn* preceeded α in *Virgo's* train, and was more southerly than the said fixed star 5' 23"; hence *Saturn's* place was in 18° 34" of *Scorpio*, with 2° 44' 8" N. Lat. supposing according to *Catal. Britan.* that α in *Virgo* is in 34° 10" of that sign, and has 2° 55' 40" Lat. The same night 17^h at *Westminster* Mr. *Desaguliers* and Mr. *Gray* observed *Saturn* preceeding the fixed star 19' with a greater S. Declin. 4' 45".

April 8. 11^h 30' at *London* *Saturn* was seen, (having lately been in opposition) but a very little more westerly than a bright telescopic star, and 5' more northerly. Whence the place of the fixed star was in 28° 18' 30" of *Libra* and 2° 41' N. Lat. and a great circle drawn thro' this star and *Saturn* seemed to be directed to a star of the fifth magnitude, omitted in *Catal. Britan.* but according to *Hevelius* in the cuspis of the northern wing of *Virgo*, and to which he affixes 26° 10' of *Libra* with 14° 43' N. Lat.

The same night 13^h 20' at *Wansted*, a perpendicular let fall from the said telescopic star on the line of *Saturn's* anse, preceeded the centre of the planet about a diameter and a half of the ring; but the star was 4' 30" distant to the south from the axis

axis of the *ansæ*; and at the same time the extremity of the eastern *ansa* was found to be in a strait line between this star and another of the same longitude which was then distant from *Saturn* $24^{\circ} 48''$ to the north; but the place of the former star was at that time in $28^{\circ} 18' 30''$ of *Libra* with $2^{\circ} 41'$ N. Lat. nearly.

September 7. at noon there was a very close conjunction of *Jupiter* and *Venus*, which our astronomers could not see by reason of clouds: But on the preceeding day in the morning, or $3^{\text{h}} 22^{\text{h}} 57' 30''$ equated time at *Wansted*, *Venus* being more westerly was distant from *Jupiter* $1^{\circ} 3' 28''$: On the 7. day at $17^{\text{h}} 21'$ *Venus* now becoming more easterly was $43' 18''$ distant from *Jupiter*; and at $17^{\text{h}} 34'$ *Venus* was more southerly than *Jupiter* $14' 23''$ the difference of declinations, and at $17^{\text{h}} 39'$ the distance of the two planets was found $44' 4''$; hence by calculation they were in conjunction on the 7. of *September* at 9. equated time, *Venus's* center being at that time but $1' 42''$ more southerly than *Jupiter's*.

September 18. in the morning at *Wansted* *Jupiter* was observed near *Cor Leonis*, with which it was in conjunction the preceeding day; *September 17.* $16^{\text{h}} 51'$ equated time, *Jupiter's* center was distant from *Cor Leonis* $24' 22''$, and at $17^{\text{h}} 6' 20''$ their difference of declination was $12' 43''$; an hour after, to wit, at $17^{\text{h}} 54'$ the distance became $24' 44''$, and at $18^{\text{h}} 7'$, the difference of declination was found $12' 35''$; hence according to Mr. Pound's computation, *Jupiter's* place is on the 17. of *September* 18^{h} equated time in $26^{\circ} 11' 7''$ of *Leo* with $45' 32''$ N. Lat.

Jan. 12. 1717 at *Westminster* Mr. *Stephen Gray* observed an appulse of the moon to four contiguous stars under the southern horn of *Taurus*, where she was observed by *Hevelius* and *Flamsteed* on *March 23. 1683* O. S. therefore, at $9^{\text{h}} 45'$ apparent time the moon appeared gibbous, and in conjunction, as it were, with the preceeding of the four stars, which is the 107th of *Taurus* in the *Catal. Britan.* and which at that time was more southerly than the moon's southern limb $1'$ and $\frac{1}{2}$; at $11^{\text{h}} 29'$ the second star which is smaller, and therefore omitted in the *Catal.* was hid a little below the middle of the obscure limb; at 12 h. 24' the third and brighter star (to wit, the 110th of *Taurus*) almost in the very time of conjunction was distant from the northern limb $6'$; lastly, at 12 h. 54' the following of the four stars (*viz.* 111th of *Taurus*) was higher than the northern limb $3' 30''$: The place of the preceeding, or the 107th star of *Taurus* according to the catalogue, was at that time in $18^{\circ} 12'$ of *Gemini* and $5^{\circ} 18'$ S. Lat. the 110th star of *Taurus* was in $19^{\circ} 26'$ and

$\frac{1}{2}$ of *Gemini*, with $4^{\circ} 44'$ S. Lat. but the following, or the 11th star of *Taurus* was in $19^{\circ} 45'$ of *Gemini* with $4^{\circ} 48'$ and $\frac{1}{2}$ S. Lat. The second small star, as appears from other observations, was at that time in $19^{\circ} 17'$ of *Gemini* and $5^{\circ} 5'$ Lat. nearly.

On the 16. of *March* following, in the morning there was a partial eclipse of the moon, which was invisible here by reason of clouds; but at *Cambridge* in *New England* Mr. *Robie* saw the beginning of the eclipse about 9 o'clock, and the end, near *Palus Maotis* at 11 h. 42' 30" pretty accurately: *Cambridge* is in Lat. $42^{\circ} 25'$, and more westerly than *London* 71° or 4 h. 44' as appears from several former observations.

Afterwards on the 9. of *September* in the evening at the *Royal Society's* house in *London* some observed the end of a lunar eclipse at 7 h. 26'; the moon rose about the middle of the eclipse, and did not emerge out of the clouds that overcast the horizon, till a little before the end.

On the 14. of *September* in the evening, for the first time the moon returned after a long interval to hide *Palilicium*; and the sky was extraordinarily clear at *London*, so that the moon and the star were seen to rise in the horizon at the same time; the immersion of *Palilicium* was at 9 h. 6' 20", the moon not being 3° high, in the very middle, as it were of the eastern limb, over against the northern part of that small *macula*, which *Hevelius* called *Stagnum Meridis*, and *Ricciolus* by his own name; it emerged a little below the middle of the obscure limb at 9 h. 58' 20": and in the twinkling of an eye shone out with all its brightness; whence likewise appears, how inconsiderable the diameter of so remarkable a star is.

On the 23. of *September* in the evening, there happened a solar eclipse scarce visible in any part of *Europe*; but we had two observations of it from our plantations in *America*, the one at *Philadelphia* Lat. 40. nearly, by Mr. *Keith* Governour of *Pennsylvania*, who saw the eclipse already begun (but that not above 1' of time) at 11^h 55'; about the middle there were about 10 digits eclipsed; the end was observed at 2 h. 46' 35" accurately: The other observation was taken at *Cambridge* in *New England* by Mr. *Robie*; the beginning of the eclipse there was at 23' post merid. at 1 h. 47' there were nine digits eclipsed; the eclipse ended at 3 h. 5' 10", when the sun was observed thro' a 24 foot telescope to have entirely emerged.

On the 5. of *December*, the moon had passed a little above *Palilicium*; Mr. *James Bradley* observed a pretty close transit; and, when the moon was very near her full he compared the star with

with that remarkable *macula*, which *Ricciolis* calls *Tycho* and *Hevelius Sina*, and from several equal distances taken with the micrometer both before and after, he concluded that *Palilicium* appeared very near the center of the said *macula* at 11 h. 15' 8" equated time at *Wansted*; at 11 h. 15' 42" *Palilicium* was distant from the nearest and southern limb of the moon 5' 55"; and *Tycho* was distant from the same limb 4' 16": At 11 h. 18' 42" the star was in a strait line with the *macula*, *Tycho* and *Copernicus*, or *Sina* and *Ætna*; and at 11 h. 25' 27" equated time it was in a strait line with *Tycho* and *Kepler*; moreover, the moon's diameter was observed to be 32' 45".

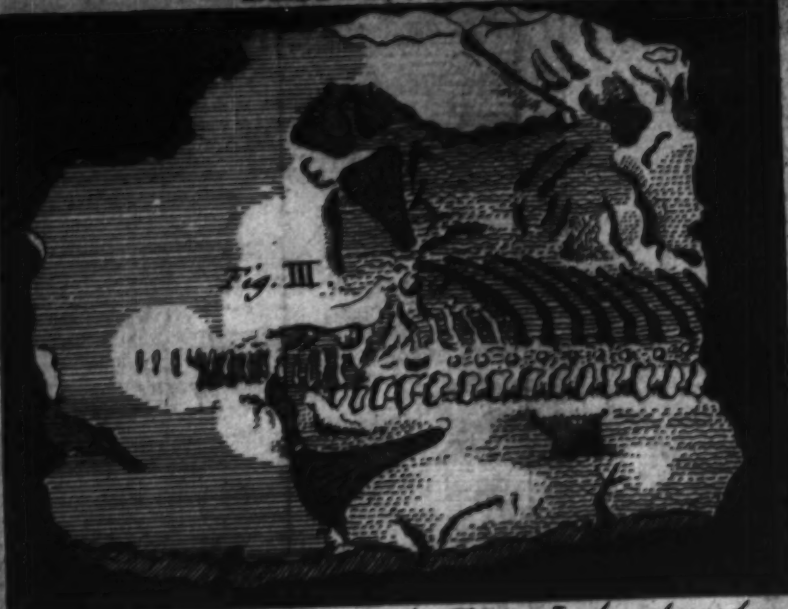
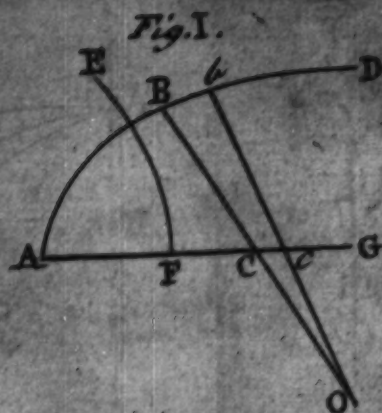
On the 29. of *January* 1718 in the evening, at *Westminster* Mr. *Desaguliers* and Mr. *Gray* watched another occultation of *Palilicium*, but being prevented by clouds, they only observed, that the star had not immersed at 5 h. 52'; but afterwards the clouds vanishing the emerfion ended at 7 h. 20', over against *Hevelius's Promontorium Sarmatiae Asiaticæ*.

On the 19. of *February* in the morning, the same observers, at the same place, the sky being overcast, could scarce see a solar eclipse; yet at 6 h. 59' they observed two digits eclipsed, and 1' after, the chord between the cusps was equal to the sun's semidiameter: But at *Wansted* Mr. *Pound* observed at 6 h. 54' 7" apparent time, that the chord between the cusps was 18' 30"; at 7 h. 17' it was 10' 18"; at 7 h. 19' 30" it was found to be 8' 5"; and the eclipse ended at 7 h. 23' 20".

On the 25. of *February* in the evening, 6 h. 44' apparent time at *Westminster*, the first star of the *hyades* in the *Bull's* nostrils (*Bayer's* γ) was observed in a strait line thro' the moon's cusps, and consequently almost in conjunction with her; it was distant from the moon's southern limb 5' 51"; the moon's diameter, as measured by the micrometer was 31' 45".

On the 28. of *February* 8 h. 36' apparent time at *Westminster*, there was an immersion of the star in *Pollux's* ham (*Bayer's* λ in *Gemini*) under that part of the moon's obscure limb, which was a little more northerly than the *macula* which *Hevelius* calls *Cresta*; the emerfion was not seen by reason of clouds; but at 9 h. 51' the star emerged over against the northern part of the *Insula Major Caspius*.

August 8. the moon rose a little below *Palilicium*, yet could not be compared with it by reason of clouds: But at *Wansted* 13 h. 2' apparent time; the preceeding of the contiguous stars to *Bayer's* σ of *Taurus* (or the last star but one in the catalogue of the *Hyades* in *Phil. Transf.* N^o 354, mark'd *q*) was seen in a strait



The Stone is 3 Foot long, and 2 Foot 2 Inches broad.

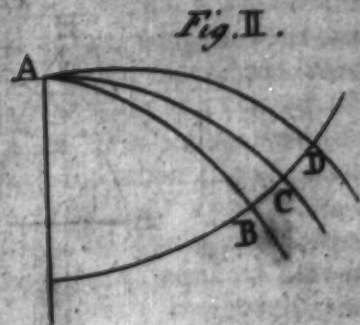
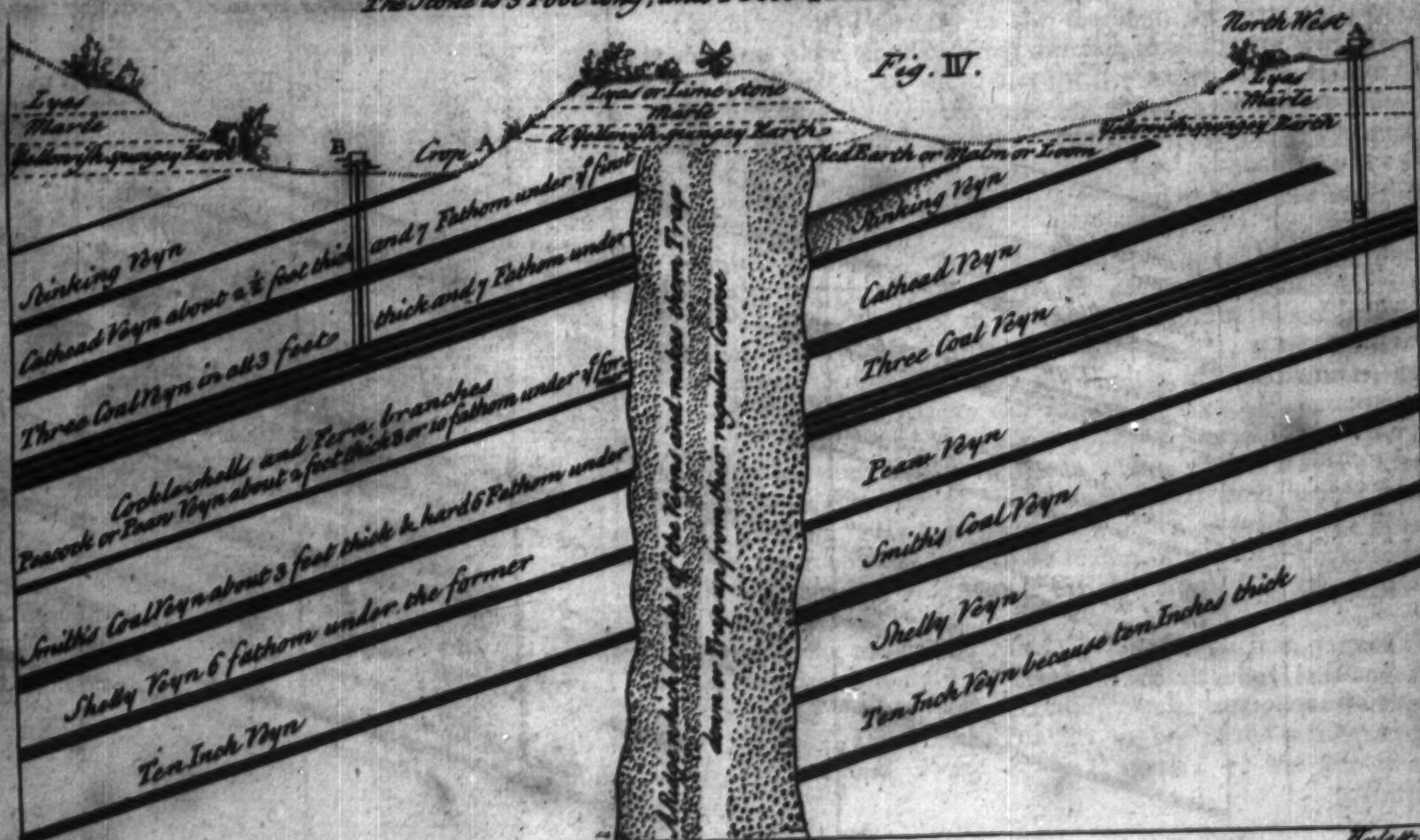


Fig. IV.



strait line thro' the moon's cusps, and distant from the south horn 4' 36": At 13 h. 7' 25" the star *p* of the same catalogue emerged a little below the middle of the dark limb.

At 13 h. 19' 4" the following of the said contiguous stars emerged, being distant from the south horn just as much as these contiguous stars are from each other, that is 7'.

August 29. in the evening, the moon being almost in her *Apo-gaum*, suffered a total and almost central eclipse; she rose when the eclipse began, of which Mr. Pound made the following very accurate observations.

Obs.	App. time	An eclipse of the moon observed at <i>Wansted</i> <i>August 29. 1718.</i>	
	h. m. s.		
1	6 53 38	The chord between the cusps measured by the micrometer	22 37
2	55 8	The same repeated	21 14
3	56 31	Repeated	19 51
4	57 49	Again	18 28
5	59 38	Again	15 00
6	7 2 41	The total immersion into the shadow	
7	8 36 13	The bright star omitted in the <i>Catal.</i> hid by the moon, below <i>Hevelius's Palus Mareotis</i>	10 2
8	8 48 18	The moon began to emerge out of the shadow	
9	9 50 13	The termination of the shadow thro' the middle of <i>Mareotis</i> , at the same time the chord between the cusps	15 0 18 28
10	53 7	The chord between the cusps	19 51
11	54 16	The same repeated	21 14
12	54 59	Again	22 37
13	56 18	Again	
14	9 0 48	<i>Porphyrites</i> emerged out of the shadow	
15	8 3	<i>Mont Sinai</i> begins to emerge	
16	9 17	The shadow thro' the middle of <i>Sinai</i>	
17	10 6	Now <i>Sinai</i> quite emerged out of the shadow	
18	11 20	The shadow thro' the middle of <i>Etna</i>	
19	17 23	Thro' the middle of <i>Corfica</i>	
20	20 0	Thro' the middle of <i>Lacus niger major</i>	
21	27 54	Thro' the middle of <i>Besbicus</i>	
22	28 45	The above-mentioned star emerged	
23	32 34	<i>Byzantium</i> and <i>Horminius</i> emerge at the same time	
24	33 58	The star had the same declination with the southern cusp of the eclipse	
25	43 28	The chord between the cusps	18 28
26	47 2	The same repeated	15 00
27	9 53 0	The eclipse seemed to end	

At 10 h. 30' the moon's diameter was $29' 45''$: Having compared these observations together, where the chords of the eclipsed part are found equal, you have the middle of the eclipse.

			middle
From the 1 and 13 observation	7	54	38
From the 2 and 12	7	55	3
From the 3 and 11	7	55	24
From the 4 and 10	7	55	28
From the 5 and 9	7	55	25
From the 6 and 8	7	55	29
Of all which the middle is	7	55	18

Mr. Folkes, with some others of the Royal Society did not less carefully observe the same eclipse in Fleet-street London, with the instruments and a very good telescope of that ingenious artist Mr. Graham.

h.			
6	38	0	The moon was with difficulty seen thro' the smoke of the town and the vapours.
6	54	13	The chord between the cusps $21' 17''$.
7	2	0	The total immersion into the shadow
7	42	15	A pretty bright star was distant from the eastern limb of the moon $19' 21''$
8	35	18	The said star was hid, being about 10' more southerly than the moon's center
8	45	30	Or, as it seemed to some, the moon began to emerge 1' later
8	49	38	<i>Palus Mareotis</i> just began to emerge
8	50	14	<i>Palus Mareotis</i> entirely emerged
9	0	5	The middle of <i>Mons Porphyrites</i> emerged
9	7	39	<i>Sinai</i> began to emerge
9	9	8	<i>Mons Sinai</i> entirely emerged out of the shadow
9	10	35	The shadow thro' the middle of <i>Mons Aetna</i>
9	12	0	<i>Mons Aetna</i> entirely emerged
9	18	51	The shadow thro' the middle of <i>Lacus niger</i> major
9	27	35	<i>Insula Besbicus</i> entirely emerged
9	42	21	The chord between the cusps $19' 9''$
9	51	25	The end of the eclipse according to some
9	52	45	The end determined from the foregoing distance of the cusps
9	56	45	The moon's diameter $29' 54''$ and again $29' 48''$

The

The shadow was exceedingly dilated, whence arose the difficulty in determining the precise moments of the emersion and end; and the darker *maculae* were plainly seen, several minutes before they could touch the margin of the shadow; and the star which was hid during the eclipse, was at that time in $17^{\circ} 16'$ and $\frac{1}{2}$ of *Pisces*, with $1^{\circ} 6' 30''$ S. Lat. nearly.

We had likewise Mr. *Derham's* observations at *Upminster* in *Essex*; those of Mr. *Wright* at *Crew* in the county of *Chester*, and those of Mr. *Hawkins* at *Wakefield* in *Yorkshire*, agreeing almost entirely with the above-mentioned, only making allowance for the difference of meridians; to wit, supposing *Upminster* $1'$ and $\frac{1}{2}$ more easterly than *London*, *Crew* $10'$ more westerly and *Wakefield* $5'$.

To conclude, we shall add a very curious observation, and as far as we know, the first of its kind, since the invention of the telescope; and which we owe to the indefatigable diligence of Mr. *James Bradley*; for, on the fifth of *September* in the morning, the sun being almost 30° high, he observed at *Wansted* a very close transit of the moon below *Palilicium*, whose distance from the nearest limb at 7 h. 59' equated time he found by the micrometer $5' 38''$: At 8 h. $17' 5''$ it was distant from the limb $1' 25''$; and the star was at 8 h. $33' 15''$ in a strait line passing thro' the moon's cusps then somewhat obtuse, and it was only $13''$ distant from the north; at 8 h. $41'$ it left that cusp $3' 42''$; and at 8 h. $45' 37''$ it was distant therefrom $5' 36''$: The moon's diameter at 8 h. $58'$ was $31' 7''$.

Of the Force of the Heart; by Dr. *Jurin*. Phil. Transf. N^o 358, 359. p. 863. Translated from the Latin.

DR. *Jurin* proposes a new solution of the famous problem, for determining the force of the heart; and in the first place, he points out the errors in *Borelli's* demonstration, and 2. He examines with the same philosophical freedom Dr. *Morland* and Dr. *Keill's* solutions thereof. 1. That the first and, indeed, the principal mistake in *Borelli's* solution seems to be his attempting to explain the force of the heart by an unactive and quiescent weight; for since the heart itself moves at the time of its *systole* or contraction, and puts into motion the bodies that oppose it, such as the blood and the coats of the arteries, it is manifest, that the quantity of its force can by no other means be determined, than by discovering the quantity of its motion; for, a motion can no more be compared with a quiescent weight, than a right line with a rectangle.

2. That in the very experiment of the mountebank, it does by no means appear, that that weight was suspended only by the contractile force of the muscles; since likewise that force, with which both the muscles that were employed, and also the cheeks, and perhaps that with which the ligaments themselves might resist the tearing and breaking asunder of their fibres, and that with which even the muscles, cut out of a dead body, sustain considerable weights, might be assisting therein.

3. That the forces of muscles of equal weight are equal; which, indeed, seems very doubtful, especially since the muscles are of dissimilar figures.

4. That he supposes the whole force of the heart, the greatest it can exert by the utmost effort of its fibres, to be employed in each *systole*; whereas the mountebank himself should he attempt to raise the weight appended either continually, or alternately with very short intervals of rest, would in a little time quite sink under the toil.

5. That he makes the resistance of the blood and arteries to be 60 times that of the whole force of the heart, instead of that force which the heart exerts in performing the *systole*, and which possibly, is the least part of the whole force.

6. That in defining that sexagecuple ratio he has been guilty of a gross mistake; for, in *Prop. 60.* instead of the ratio which the sum of the powers P and Q has to the sum of the powers R and S, he has taken the ratio between the rectangle of the powers P and Q, and the rectangle of R and S; which error if corrected by the following propositions, in *Prop. 73.* the resistance will be much greater, than *Borelli* has made it, to wit, a weight of 1,076,000 pounds, instead of 180,000, and that according to *Borelli's* own suppositions.

7. Lastly, that he would obtrude upon his readers as a miracle or prodigy, this weight of 180,000 pounds, since it is overcome by the force of the heart equal to 3,000 pounds; and that he would call in to his assistance the force of percussion, as a supernatural cause; for, in reality it is the same thing as if a weight of 3,000 pounds did equiponderate another of 180,000, appended at a subsexagecuple distance from the centre of a ballance of unequal arms.

Dr. *Jurin* forbears mentioning some smaller mistakes and several hypotheses both entirely arbitrary and repugnant; and these, or at least the greater part, are not he thinks to be imputed to this learned man, but rather to be pardoned in a posthumous performance,

Dr. Mor-

Dr. Morland, in a treatise published in *English* about the force of the heart, has shewn a very ingenious method of bringing this force to experiment; but besides the above-mentioned mistake in *Borelli*, as his comparing the force of the heart with a quiescent weight, he seems also to be faulty in supposing the whole action of the heart to be expended in dilating the coats of the arteries; whereas, the heart does not only distend the arteries, but likewise propel the blood with a certain degree of velocity thro' the whole system of the veins and arteries.

It now remains to consider Dr. James Keill's solution, published in his *Medico-physical* essays on the animal oeconomy; who was the first that not only rejected the force of the heart, as determined by *Borelli*, and unanimously received and approved by authors, but substituted another infinitely less in its stead; but besides the first mistake in *Borelli's* solution, he has likewise fallen into the following; that either he did not well understand, or at least not properly apply the *Newtonian* corollary he makes use of in determining the force of the heart; for, the weight, according to Sir *Isaac Newton*, whereby the motion of water running out of a vessel may be generated, does by no means produce the motion of the water, it acquiring this in its fall, by the force of gravity; but this weight falling in a given time, produces a motion equal to that of water running out in the same given time.

Besides, Dr. Keill supposes the velocity of the blood flowing out of the heart to be always equal during the whole time of the *systole*; but this will appear anon to be considerably unequal.

In that more simple method he afterwards makes use of, besides the errors already mentioned, he also falls into two others; for, 1. He supposes the force of the heart in different animals to be in the ratio of their respective weights, which Dr. *Jurin* demonstrates to be false. 2. He supposes the velocity of the blood flowing out of the *iliac* artery when cut, to be the same with that with which the blood is thrown out of the heart into the *aorta*; but since almost the whole quantity of blood thrown out of the heart is discharged by the other *iliac* artery when cut, it is manifest, its velocity in the *iliac* artery is so much greater than in the *aorta*, as a circular section of the *iliac* is less than a section of the *aorta*; besides that, the equable velocity, with which blood flows thro' the *aorta*, is very different from that with which it goes out of the heart.

In the same manner almost may the insufficiency of his method be shewn for defining the ratio between the different velocities

cities of the blood flowing thro' the *aorta*, in the different cases of a resistance or none: But since by that experiment, not only one but both velocities are found greater than they should be; whence the ratio between them, may not be much disturbed, the proportion laid down by Dr. Keill may be pretty safely assumed, as near the truth.

Having thus discovered the errors of others, Dr. Jurin lays down (in order to prevent any ambiguity) the exact state of the question.

By the force of the heart he means, either the motion of the heart, whilst in *systole* or contraction; or the motion of any weight, which, opposed to the blood flowing out of the heart, and carried with a proper degree of velocity towards the contrary parts, can with an equal force equiponderate and stop the efflux of the blood, and consequently, the contraction of the heart itself.

Since it is hardly to be expected, that we can determine the force of the heart *a priori*, because we are not thoroughly acquainted either with its inward contexture, the nature of the contracting cause, or its force, it remains, that we estimate that force by its effects, or *a posteriori*.

The whole action of the heart consists in the contraction of its ventricles; but the ventricles in their contraction impinge upon the blood, and communicating a part of their motion thereto, push and throw it out with great force; the blood, protruded thus into the arteries, as the *aorta* and pulmonary artery, and tending every way, impinges partly upon the coats of the arteries, that are now collapsed and flaccid after their *systole*, and partly upon the preceeding blood, that flows more slowly; whence the coats of the arteries are gradually distended, and the antecedent blood is accelerated: If we imagine the arteries divided into very small transverse sections; by the first small portion of blood, that flows out of the heart into the first section, that section is partly distended, and the blood that before was contained therein is partly protruded into the next section, which is thereby distended and this action is continued thro' all the succeeding sections of the arteries; afterwards the second and third portions of the blood and then the succeeding ones are thrown into the first section of the artery, which they thereby dilate a little more, and successively propel the blood, that was before contained therein, into the next sections; and thus is continued, till all the blood be thrown out of the ventricles: But this we are to observe, that the arteries, the more contracted and flaccid the

they become, the less they resist their dilatation; but the more they are dilated; the more strongly they resist any further dilatation; and therefore, that the force of the blood flowing out of the heart is at first more employed in distending the arteries, than in protruding the antecedent blood; but that at last the preceeding blood is propell'd more than the arteries are distended; for, now being rigid and stiff, they scarcely admit of any farther dilatation.

Since the blood, flowing out of the heart, communicates, as has been already said, a part of its motion to the coats of the arteries, and a part to the preceeding blood, it must necessarily lose of its former velocity; consequently, while it stops the contraction of the ventricles, it receives a new impulse from them, part of which it expends, in the same manner as before, on the coats of the arteries and the preceeding blood; whence it is again retarded; and receives another impulse from the ventricles, and so on, till all the blood be thrown out of them.

Besides the above mentioned cause, there remains another, whereby the blood flowing out of the heart is gradually retarded, and consequently, receives new successive impulses from the contraction of the ventricles; for, tho' we suppose the blood, flowing into the *aorta*, to meet with no resistance at all, and consequently suffer no diminution of its motion; yet since it is conveyed from a wider into a narrower channel, it continually encreases in length, till it come all, into the *aorta*; and since the section of the *aorta* is not diminish'd, the velocity of the blood necessarily must; for, the motion of the blood is in the compound ratio of the section of the *aorta*, the velocity therein, and the length of the column of blood by Dr. *Jurin's Theor.* 3. of running waters; but since that portion of the blood, which is already come into the *aorta*, is gradually retarded, the blood, still contain'd in the ventricle, will be thence retarded, and consequently, the contraction of the ventricle itself; whence the ventricles will continually communicate different parts of their motion to the contiguous blood, that is incessantly retarded upon these accounts: Hence it appears, to observe this by the bye, that the motion of the blood flowing out of the heart is different from its motion, when now thrown out of it, and actually running in the arteries; as also, that the stroke or impulse of the ventricles upon the blood, that otherways would be but one, and performed in a moment of time, is by means of the above-mentioned causes, by which the

the blood is continually retarded, continued thro' the whole systole of the heart.

We may therefore consider either of the ventricles of the heart, impelling the blood, as a given body, impinging with a given velocity upon another body at rest, and to which communicating a part of its motion, both bodies are carried with a common velocity: but its force is equal either to the product of the weight of the ventricle and its initial velocity, before it impinges on the blood; or to the sum of the motions of the ventricle, and effluent blood, and of the motion communicated to the coats of the arteries, and to the preceeding blood; or even, supposing all resistance of the arteries and preceeding blood remov'd, to the sum of the motions of the ventricle and effluent blood.

Theorem. 1. *The motion, with which a hollow engine unequally contractile, is made to contract, is equal to the sum of the products of each partile of the engine into its respective velocity: This appears from mechanics.*

Corol 1. The motion of the engine is less than the product of the weight of the engine into the velocity of those parts thereof, which in the time of contraction move the swiftest.

Cor. 2. The motion of the engine is equal to the product of its weight into some mean velocity between the velocities of those parts of the engine that move swiftest, and those that move slowest.

Cor. 3. If many similar engines contract similarly with a mean velocity, either equable or unequable, yet similarly increas'd or diminish'd in all the engines; the motion, with which each engine is contracted, is in the compound ratio of the quadruplicate ratio of the homologous diameter of the engine, and the inverse ratio of the time in which the contraction is performed; or the compound ratio of the weight of the engine, the subtriplicate ratio of the same weight, and the inverse ratio of the time.

Theor. 2. *If by contraction water be forced out of a hollow engine as ABCD Plate X. Fig. 11. unequally contractile, the motion of the water, running out of the orifice A, is equal to the sum of the products of any transverse sections of all the filaments of water AB, AC, AD, each into its respective length and velocity.*

Demon-

Demonstration. Instead of the filaments of water, suppose the engine to consist entirely of very small tubes, of unequal sizes, as A B, A C, A D, terminating in the orifice A: The motion of the water in any tube is equal to any section of the tube multiplied into the velocity of the water, running thro' that section, and into the length of the tube by *Theor. 3. of running waters*: Consequently, the sum of the motions of the water in all the tubes taken together, or the motion of the water running out of the orifice of the engine, is equal to the sum of the products of the sections of all the tubes or filaments of water into their respective lengths and velocities. Q. E. D.

Cor. 1. The motion of the effluent water is less than the product of the orifice A, the velocity of the effluent water and the length of the longest filament of water; for, the product of the orifice into the velocity of the effluent water, is equal to the sum of the products of each section of the filaments into its respective velocity; and the sum of these products multiplied into the length of the longest filament of all, is greater than the sum of the same products, each multiplied into its respective length.

Cor. 2. The motion of the water is equal to the product of the orifice A, and the velocity of the effluent water into the mean length between the longest and shortest filaments; or it is equal to the product of the quantity of water, running out in a given time, and of the aforesaid mean length, applied to that given time.

Cor. 3. If several similar engines full of water be similarly contracted, with either an equable, or unequable mean velocity, yet similarly increas'd or diminish'd in all the engines; the motion with which the water runs out of the orifice of any of them, is in a ratio compounded of the quadruplicate ratio of any homologous diameter of the engine, and the reciprocal ratio of the time, in which the contraction of the engine is performed; or in a ratio compounded of the ratio of the weight of the engine, or the bulk of water, that is either contained in the engine, or has run out of it, the subtriplicate ratio of the same weight or bulk, and the reciprocal ratio of the time.

Problem, To find the force of the heart.

Let p be $\equiv$ the weight of the left ventricle, or the quantity of blood equal to the same weight; S $\equiv$ its internal superficies; l $\equiv$ the mean length of the filaments of blood run-

ning out of it; s = the section of the *aorta*; q = the quantity of blood, contained in the left ventricle; t = the time in which the blood would be thrown out of the heart, upon removing the resistance of the arteries and antecedent blood; v = the variable velocity, with which the blood running out of the heart would flow thro' the *aorta*, upon the removal of the resistance; x = the variable length of the *aorta*, run over by the blood flowing out of the heart; z = the time in which the length x is run over.

Hence the mean variable velocity of the blood, contiguous to the ventricle, or the mean velocity of the ventricle itself is $= \frac{\int v}{s}$.

The motion of the ventricle is (by *Theor. 1. cor. 2.*) $= p \times \frac{\int v}{s}$.

The motion of the effluent blood is (by *Theor. 2. cor. 2.*) $= \int v \times \frac{1}{1+x}$.

The sum of all these on the force of the ventricle is $= \int u \times \frac{p}{s} + \frac{1}{1+x}$; but $\frac{1}{1+x}$ is $= \frac{x}{2}$; whence the force of the ventricle by Sir *Isaac Newton's* inverse method is $= \frac{\int x}{2}$

$\times \frac{p}{s} + \frac{x}{2} + l$; but when x is $= t$, $\int x$ will be $= q$; hence the force of the ventricle is $= q \times \frac{p}{s} + \frac{q}{2s} + l$.

In the like manner is the force of the right ventricle found $= \frac{q}{s} \times \frac{\pi}{2} + \frac{q}{2s} + \lambda$; for, the same things in the right ventricle are express'd by *Greek* characters, which in the left ventricle are by *Roman*.

Hence the whole force of the heart is

$$= \frac{q}{s} \times \frac{p}{s} + \frac{\pi}{2} + \frac{q}{2s} + \frac{q}{2s} + l + \lambda. \text{ Q. E. D.}$$

If we suppose $p = 8$ ounces *Avoirdupois* = 13.128 cubic inches; $\pi = 4$ ounces *Avoird.* = 6.564 cubic inches; $s = 10$ square inches; $\Sigma = 10$ square inches; $l = 2$ inches; $\lambda = 1$ inch

1 inch and $\frac{1}{4}$; $q = 2$ ounces *Avoird.* = 3. 282 cubic inches, $S = 4$.185 square inches, and $\sigma = 0$. 583 square inches according to Dr. Keill's experiments; $t = 1^{\circ}$.

The force of the ventricles will be equal to the motion of the following weights, to wit, that of the left ventricle will be nine pounds one ounce; of the right ventricle, six pounds three ounces; of the whole heart, 15 pounds four ounces; the velocity of which weights is such as that they may run over the length of an inch each second of time.

Cor. 1. As often as the pulse becomes quicker; either the resistance is diminish'd, or the force of the heart increas'd, or a less quantity of blood than usual is thrown out of the heart at each systole or contraction.

Cor. 2. If the pulse be slower than ordinary; either the resistance must necessarily be increas'd, or the force of the heart diminish'd, or a greater quantity of blood be thrown out of it.

Cor. 3. If the resistance be increas'd, either the pulse will be necessarily retarded, or the force of the heart increas'd, or a less quantity of blood than usual be thrown out of the heart.

Cor. 4. If the resistance be diminish'd, either the pulse is accelerated, or a greater quantity of blood than ordinary is thrown out in each systole, or the force of the heart is diminished.

Cor. 5. If the force of the heart be increas'd, either the resistance will necessarily be increas'd, or the pulse accelerated, or a greater quantity of blood be thrown out of the heart.

Cor. 6. If the force of the heart be diminished, either the resistance must be necessarily diminish'd, or the pulse become slower, or a less quantity of blood be thrown out of the heart.

Cor. 7. When a less quantity of blood is thrown out of the heart; either the pulse is accelerated, or the force of the heart diminish'd, or the resistance is increas'd.

Cor. 8. When a greater quantity of blood is thrown out of the heart; either the pulse will become slower, or the force of the heart be increas'd, or the resistance be diminish'd.

Scholium 1. Since it seems very difficult, accurately to determine the internal superficies of the ventricles, or even the ratio of their diminution in contraction, the Dr. has thought it sufficient to estimate them nearly; since if you suppose each of them equal either to 12 or 8 square inches, there will thence arise but a very inconsiderable change in their force; and the same thing may likewise be observ'd of the mean length of the filaments of the blood: Besides, the differences of the sections of both arteries, and their branches immediately

issuing from the heart, are neglected, as being very difficult to estimate, and almost insensible; otherways the force of the heart must have been so much less than it is determined above.

Schol. 2. Dr. James Keill has determined the velocity of the blood flowing out of the heart, the resistance being remov'd, to be nearly such, as to run over six foot and $\frac{1}{4}$ in each second of time; but he supposes, the velocity of the blood to be equable all the time of the systole, whereas Dr. Jurin has shewn above, that it is considerably unequal, and continually retarded from the beginning of the systole: If any one would determine this velocity, let him substitute the force of the ventricle last found, in the fourth equation above set down, and let x be suppos'd of any value, in order to find v or the velocity corresponding thereto; thus, since at the beginning of the systole, x

is 0, and at the end x is $= \frac{9}{5}$; hence is determined the velocity with which the blood at the beginning of the systole may run over 14 foot and $\frac{1}{4}$ in a second of time, and at the end 4 foot and $\frac{1}{4}$ in the same time: And in like manner in the right ventricle: With the initial velocity the blood will run over about 10 foot and $\frac{1}{4}$ in a second of time, and with the final 3 foot in the same space of time.

Dr. Jurin has hitherto applied that hypothesis, wherein the muscles, constituting the ventricles of the heart, acquire in a moment of time all their motion of contraction: But if we suppose their motion communicated, not in a single moment, yet in so very small a space of time, as compar'd with the whole duration of the systole may have an inconsiderable ratio thereto, the force of the heart must be suppos'd somewhat greater than it is determined above; but if that motion be suppos'd, in the progress of the systole, to encrease in the ratio of the time, the whole quantity of motion, acquir'd at the end of the systole, will be twice greater than is suppos'd above, when there is no resistance to the blood flowing out of the heart; but when there is the usual resistance, the same will be five times greater, as will easily appear upon making the calculation: In like manner the Dr's calculation may be accommodated to any other hypothesis, wherein the motion of the ventricles is increas'd in a duplicate or any other higher ratio of the time; but the force acquir'd at the end will be found much greater than is suppos'd above; to wit, from a duplicate ratio a triple force, from a triplicate a quadruple, from a quadruplicate a quintuple, and so on *in infinitum*.

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The second hypothesis, in which the ventricles acquire their whole quantity of motion in a very small space of time, seems to the Dr. more probable by far than any of the rest; since some time must necessarily be spent in producing any motion, and since the motion of the ventricles does not seem to increase so slowly, but that it may be increas'd with a greater velocity than in the ratio of the time; for, the motion of the muscles cannot be performed by the sole *impetus* of any fluid proceeding from the blood; since with either arm we may produce a much greater motion than that of the blood flowing thro' all the vessels of the body; it remains therefore, that the fibres of the muscles, of which the ventricles of the heart consist, be put into motion by a certain kind of rarefscency of the liquors flowing into them; for, as often as it acquires a great force, it is for the most part sudden and almost instantaneous: Add to this, that the motion of the ventricles is much less by this hypothesis, than by the third: For, the wise author of nature never employs a greater force, than is sufficient for obtaining the end.

But whether this hypothesis be admitted, or any other of the above-mentioned, all the Dr's corollaries may be equally deduced from the problem; and whether they may afford any assistance in explaining the history of diseases must be left to the sagacious physician; but it may be easily known from the nature of each disease, whether the resistance be increas'd or diminish'd; and it is probable, that the force of the heart is increas'd or diminish'd upon the increase or diminution of the force of the rest of the muscles; tho' the celebrated *Bellini* has determin'd otherways.

Theor. 3. The whole motion of resistance to the blood, flowing out of the heart during the systole; or the whole motion communicated to the preceeding blood and to the coats of the arteries, is very nearly equal to the whole force of the heart.

Demonstr. After each systole of the heart, that part of the *aorta* and pulmonary artery next to it, remains full of blood, during the whole systole of the arteries: For, neither their make or connection with the heart suffer the entire collapsing of their coats, or their cavity to be without blood; otherways, upon the contraction of the remaining parts of the arteries, the blood contained therein, would be impell'd back into that empty

empty cavity with a motion, both useless and contrary to the natural motion of the blood; and in that case the semilunar valves would not be distended towards the ventricles, and consequently, the blood thrown out of the auricles into the ventricles, even in the *diastole* of the heart, would be protruded into the arteries.

Hence it appears, that the last quantity of blood, thrown out of the heart after each *systole*, continues immoveable in the arteries; and consequently, has both received the whole motion of the ventricles, and likewise communicated the same partly to the antecedent blood, and partly to the coats of the arteries. Q. E. D.

Theor. 4. The motion communicated to the preceeding blood in the systole is, to that communicated to the coats of the arteries, very nearly as the time of the systole to that of the diastole.

Demonstr. Since the blood is carried with an equable motion thro' all the vessels of the body, excepting those parts of the arteries nearer the heart; both that motion, which the blood loses by its friction against the sides of the vessels, and likewise that communicated to it by the *systole* either of the heart or arteries, must necessarily be equal in equal times; but the motion, communicated to the blood by the *systole* of the arteries, is precisely the same with that first impressed upon the coats of the arteries by the *systole* of the heart; since the arteries are contracted with the very same *impetus* with which they were dilated; and since the *systole* of the arteries continues the same time with the *diastole* of the heart, &c. Q. E. D.

Corol. If with Dr. Keil we suppose, the *systole* of the heart to be performed in $\frac{1}{3}$ of the time between two pulsations, the motion communicated to the preceeding blood will be $\frac{1}{3}$ of the whole force of the heart, and the motion communicated to the arteries will be double the former or $\frac{2}{3}$ of the whole force of the heart.

Theor. 5. The force of the heart in different animals is in a ratio compounded of the quadruplicate ratio of any homologous diameter of the animal, and the inverse ratio of the time in which the heart is contracted; or, in a ratio, compounded of the ratio of the weight, either of the heart itself or of the whole animal, the subtriplicate ratio of the same weight, and the reciprocal ratio of the time.

This is easily demonstrated, either from *Corol. 3. Theor. 1* and 2. or from the power of the heart, as determined by the preceeding problem.

Cor. 1. If we suppose the force of the heart to be in the ratio of the weight either of the heart itself or of the animal, or of the whole quantity of blood in the animal; the length of the animal

mal will be in the ratio of the time in which the *syssole* of the heart is performed, or in the inverse ratio of the frequency of the pulsation.

Cor. 2. If the ratio of the length of the whole animal be greater than the inverse ratio of the frequency of pulsation, the ratio of the force of the heart must necessarily be greater than the ratio of its weight.

Scholium. Since it is evident by experiment, that the pulsations in children are by so much less frequent than those in grown people, as children are shorter than they. We must conclude by *Cor. 2.* that the force of the heart of a grown person is in a greater ratio to the force of a child's heart, than is the ratio of their weights; and the same holds in all other muscles; for, should the strength of the body be in the ratio of its weight, children might dispatch equal spaces in the same time with grown persons.

In the same manner, as the motion of the blood, flowing out of the heart, was determined by *Theor. 2.* so in like manner may that of the urine flowing out of the *Urethra*; by supposing the length of the *Urethra* and bladder to be 12 inches, and two ounces of urine to be discharged in a second of time; for, in that case the motion of the effluent urine will be equal to the motion of a weight of one pound and a half, which runs over the length of an inch each second of time: But since the urine is discharged not only by the contractile force of the bladder, but likewise by the concurrence of the diaphragm and abdominal muscles, the force of the bladder cannot be estimated by the motion of the urine.

An Account of a contagious Distemper among Cows near London; by Mr. Bates. Phil. Trans. N° 358. p. 872.

ABOUT the middle of *July* 1714. the distemper appearing at *Islington*, and the Lords Justices having notice thereof, ordered Mr. *Bates* to examine into the truth of the report of its being contagious; and the Lord Chancellor having granted such authority as was proper to make the discovery, four Justices of the peace for the county of *Middlesex* were also appointed to make the necessary examinations.

Pursuant to those orders they went to *Islington*, and there they found, that Mr. *Ratcliff* had lost 120 cows out of 200; Mr. *Rusford* 62 out of 72; and Mr. *Pullen* 38 out of 87: Mr. *Ratcliff* gave the following account of this distemper; the cows at first refused

refused their food; the next day they had huskish coughs, and voided excrement like clay; their heads swelled and sometimes their bodies; in a day or two more there was a considerable discharge of a mucous matter by the nose, and their breath had an offensive stench; and lastly, they had a severe purging, which was sometimes bloody, and which terminated in death; that some died in three days, and others in five or six, but that the bulls lived eight or ten days; that all the time of their illness, they refused all manner of food and were very hot.

Several of the *cow-leeches* or Doctors agreed, that it was a murrain, or rather a plague; and all the methods, they had tried for a cure, proved unsuccessful.

Mr. *Bates*, being ordered by the Lords Justices to deliver in writing what should be proper to be done, drew up the following proposals.

1. That all such cows as are now in the possession of Mr. *Ratcliff*, *Rufford* and *Pullen* be bought, killed and burnt; or at least, that the sick be burnt; and those, that are well, kept and secured on the grounds where they now are, that such of them as sicken or die of this distemper may be burnt.

2. That the houses, in which those sick cows have stood, be washed very clean, and then smoaked by burning of pich, tar and wormwood, and be kept three months at least before any other cows be put into them.

3. That the fields, where those sick cows have grazed, be kept two months before any other cows are suffered to stand or graze thereon.

4. That the persons, looking after such cows as are ill, should have no communication with those that are well.

5. That the same methods be observed, if any one of the cow-keepers should get this distemper amongst them; and that as soon as they perceive any of their cows to refuse their meat, or have any other symptoms of this distemper, that they immediately separate them from the rest, and give notice to such persons as the Lords Justices shall appoint, that they may be burnt, and that the places where they have stood or graz'd on be ordered as above.

6. That the cow-keepers be required to divide their cows into small parcels, not above 10 or 12 to be in a field together.

The four Justices of the peace had orders to comply with the preceeding proposals, and to allow 40 s. for every sick cow that was burnt, belonging to Mr. *Ratcliff*, *Rufford* and *Pullen*: But the

the free intercourse, which both masters and servants had had with each other, cows, had spread the contagion; and the distemper began soon to appear in several other neighbouring places.

The Gentlemen then summoned all the cow-keepers in the county, and acquainted them with the above-mentioned proposals, and offered them 20 s. for every cow they burnt, that had not been sick above 24 hours; but for such as had been longer ill, or were dead, they would allow no more than the value of their skins and horns.

Upon dissecting 16 cows in different degrees of infection, Mr. Bates found the putrefaction of their *viscera* to increase in proportion to the time of their illness. The first five he opened, had bled with those that were ill, and the symptoms of this distemper were just become visible; in these the gall-bladders were larger than usual, and replete with bile of a natural taste and smell, but of a greener colour; their *pancreas* were shrivelled, some of the glands obstructed and tumefied; several of the glands in their mesenteries were twice or thrice their natural bigness; their lungs were somewhat inflamed, and their flesh felt hot. All other parts of their *viscera* appeared as in a healthful state.

The next six he opened, had been ill about two days; in these the livers were blacker than usual; and in two of them, there were several cysts replete with a petrified substance like chalk, and about the bigness of a pea; their gall-bladders were twice their usual bigness, and filled with bile of a natural taste and smell, but of a greener colour than the first; their *pancreas* were shrivelled; some of their glands were very large and hard, and of a blackish colour; their lungs were inflamed and had several small cysts forming; their intestines were full of red and black spots; their flesh was very hot, tho' not altered in colour.

The five last he opened were very near dying; in these he found the liver blackish, much shrivelled and contracted; and in three of them, there were several cysts as big as nuts or nutmegs, filled with a petrified substance like chalk; their gall-bladders were about three times their usual bigness, and filled with bile of a natural taste and smell, but of a deep green colour; their *pancreas* were shrivelled and contracted; several of their glands were very large and hard, and of a black colour; several of the glands in their mesenteries were distended to 8 or 10 times their natural bigness, and very black; in the *pelvis* of most of those glands in two cows there was a yellow petrification, of the consistence of a sand-stone; their intestines were of the colour of a

snake, their inner coat being excoriated by purging; their lungs were much inflamed, with several cysts containing a yellow purulent matter, several of them as big as a nutmeg; their flesh was exceeding hot, tho' very little altered in colour.

Mr. Bates has here only given a general account of his dissections, in the three different stages of the distemper, in which the difference was but inconsiderable.

The three following cases were very extraordinary; for, in one of them the bile was petrified in its vessels, and resembled a tree of coral, but of a dark yellow colour and brittle substance: In another there were several inflammations on the liver, some as big as half a crown, cracked round the edges, and appeared separating from the sound part, like a pestilential carbuncle: In a third, the liquor contained in the *pericardium* (for lubricating the heart in its motion) resembled the sediment of *aqua calcis*; and had excoriated and given as yellow a colour to the whole surface of the heart and *pericardium*, as *aqua calcis* could possibly have done.

In giving his opinion of this distemper, Mr. Bates premises, that all cows have naturally a purgation by the *anus* for five or six weeks in the spring, from the *firmness* of the grass, as the cow-keepers term it; during which time they are brisk and lively, their milk becomes thinner and of a bluish colour, sweeter to the taste and in greater quantity; but the spring, preceeding this distemper, was so dry all over *Europe*, that the like was not known in the memory of any one living; the consequence of which was little grass, and that so dry and without that *firmness* it has in other years, that he could not hear of one cow-keeper, who had observed his cows to have that purgation in the same degree as usual; and very few who had observed any at all: They all agreed, that their cows had not yielded above half the quantity of milk that summer they did other summers; that some of them were almost dried up; that the milk they did give was much thicker and yellower than in other years: It was observed by the whole town that very little of the milk, then sold, would boil without turning; and it is a known truth, that the weakest of the common purges you can give a cow entirely takes away her milk: From all which circumstances, and several others of less moment that occurred, during his daily conversation at that time with cow-keepers, &c. he thinks it evident, that the want of that natural purgation was the sole cause of this disease; by producing those obstructions, which terminated in a putrefaction and made the distemper contagious.

Cows are likewise subject to a purgation (tho' in a less degree), from the same quality in the grass, about the latter end of *September*, which is called the latter spring; and which *Mr. Bates* supposes contributed considerably to the preventing the increase of this distemper; for this purgation coming on so soon after the disease appeared, it is not unreasonable to suppose, but it freed such cows, as were not much injured from the bad effects of those obstructions, occasioned by the want of their vernal evacuations.

Several physicians attempted the cure of this distemper, and made several essays to that purpose; but by the dissections *Mr. Bates* was convinced of the improbability of their succeeding: However, he made trial with the following *recipe* and directions from *Holland*, said to have been used there with good success; but the effect was answerable to his expectation; for, in a vast many instances he was not sensible of the least benefit.

R. Herb. Aristoloch. rotunda

Veronica aa M. VIII

Pulmonariae,

Hyssopi,

Scordii, aa M. 4

Rad. Gentiane,

Angelicae,

Petasitidis,

Tormentillae,

Carlinae, aa lb. ſs

Bacc. lauri

Juniperi, aa ʒxii. *Misce fiat pulvis. vide*

Phil. Trans. N° 338. in fine.

An ounce of this powder at a time is to be given in water three or four mornings successively; then intermit for four days; and if the distemper continue, repeat the powders in warm water as before.

There was no one method in practice, but was tried on this occasion, tho' none of them was attended with any appearance of success, excepting that of bleeding plentifully, and giving large quantities of cooling and diluting liquids; but even by this method, the instances of success were so few, that they do not deserve any further notice.

The Lords Justices being informed, that the feeding cows with distillers grains was a new custom, and the cause of this

disease, order'd Mr. Bates to enquire into the truth of it; but he found it to have been the practice of several of the cow-keepers upwards of 20 years, without the least appearance of any inconvenience; and that some of those who had suffered most, had never given any; nor is there any difference between those of brewers and distillers, only that the latter are drier.

It was likewise said, that the want of water was the cause of this disease, the springs and places where people used to water their cows, being almost every where dry; and many were obliged to send them several miles for water: This might produce some diseases, but such only as they could get by the fatigue of being driven so far; for, Mr. Ratcliff, Mr. Rufford, and Mr. Pullen had the new river water running thro' the very grounds, where their cows constantly graz'd; and so had most of the cow-keepers at Islington: There were at that time several other reports of the cause of this distemper, but none that had any shew of reason.

About the latter end of *September* the disease increased, and the numbers brought to be burnt were so great, that it could not well be executed; therefore, it was judged proper to bury them 15 or 20 foot deep, first making large incisions in their most fleshy parts and covering them with quick-lime: Such as had sick cows brought their calves to be buried, and were allowed from 5 to 10 s. the calf.

In the beginning of *October* being informed, that some of the cows in *Norfolk*, *Suffolk* and *Hertfordshire* had catched this distemper, and apprehending, that it would become general, Mr. Bates gave in the following report to a Committee of council.

The distemper among the cattle increasing and beginning to appear in several other counties, I thought it my duty to acquaint your Lordships with the hazard that may attend their not being duly buried: It is the opinion of all physical authors that treat of contagious diseases, as well as of several of the physicians in *London*, that a putrifaction of so many cows, as there is reason to fear will die of this distemper, may produce some contagious disease among men; unless they are buried so deep under ground, that the infectious effluvia cannot injure the air, which I am certain has very rarely been complied with, excepting in the counties of *Middlesex*, *Essex* and *Surry*; the Gentlemen employed being capable of acting in these counties only: It is affirmed by several now living, that there was a mortality among the cattle a little before the last
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great plague in 1665, which was imputed to the want of a due care in burying them: And your Lordships may know of what importance this was judged by the king of *Prussia*, the states of *Holland*, and several other princes and states, by the care they took to publish decrees and placarts, commanding them to be buried upon pain of death, or other severe penalties; and I humbly conceive it would be necessary, not only to bury such as shall die, but likewise such as are already dead; as also that they be buried 5 or 10 foot deep at least.

Their Lordships thought fit to defer all proceedings upon this report, till the distemper, becoming more general, should make it necessary; but that necessity never happened; for, within three weeks or a month after, the following particulars concurr'd to put an end to the distemper.

The cows began their latter purging, which contributed much to prevent the distemper from appearing in fresh places; and the cow-keepers were convinced, that the distemper was incurable. The knowledge of the distemper was spread all over *England*; so that none would buy a cow in the country; and the Gentlemen prevented their being killed in town, by having the markets examined daily; and such meat condemn'd, as appeared suspicious: The cow-keepers now divid-ed their cows into small parcels, whereby they lost only that in which the distemper happened, whereas, before that method, when one cow catch'd that distemper, if she herded with 1, 2 or 300, the contagion was such, that scarce one escaped: Those who had no sick cows avoided all communication with such as had: They likewise found, that the keeping their cows so long when ill, had been the chief cause of their loss; they, therefore now brought them to be buried upon the first appearance of the distemper, before the contagion could possibly have got to any great height.

The severity of this distemper in *England* did not last above three months; tho' it was not entirely suppress'd till about *Christmas*: But in several other countries it continued two or three years; and Mr. *Bates* was credibly informed, that it still raged in *Holland* with as much violence as ever; and that they lost in cows, oxen and bulls, upwards of 300,000.

The divine providence has so dispos'd the matter of animal bodies, as to render contagious diseases very seldom infectious to different species; but experience plainly shews, that contagions may be communicated to the same species, by touching the woollen, linen, &c. to which the infectious effluvia of the diseased

diseased had adhered, tho' the two bodies should be at a very great distance from each other; and Mr. Bates supposes, that more hundreds died from the infection, which was carried by the intercourse the cow-keepers had with each other, than single ones by the original putrifaction.

The number of cows and bulls lost by this distemper in the counties of *Middlesex, Essex and Surry*, were 5418; and of calves 439.

The loss the cow-keepers sustained, as delivered in upon oath was 24500*l.* allowing 6*l.* per cow, which at a medium was no more than their prime cost: Tho' his majesty granted a brief for the 24500*l.*; yet the many false reports that were then industriously propagated, to lessen the value of the cow-keepers losses, so frustrated that charity, that the entire sum collected (after first paying the charges of collecting) was but 6278*l.* 2*s.* 6*d.* which on a dividend amounted to 5*s.* and three half-pence in the pound, computing their loss as above at 6*l.* per cow; tho' if we consider their contracts with brewers for grains, their rent of grounds which lay useless, servants wages, &c. their real loss may (by a modest computation) be allowed to be 10*l.* for each cow that died.

Some accurate Observations of the transit of the Body and Shadow of Jupiter's fourth Satellite over the Disk of the Planet; by Mr. Pound. Phil. Trans. N^o 359. p. 900.

FEB. 16. 1719. at 6^{*h*} 15' we saw thro' a short tube all the four satellites, the three outermost on the east side of *Jupiter*, and the innermost near the western limb, approaching to an eclipse: The fourth at that time was about half a semidiameter of *Jupiter* from the eastern limb: Then it prov'd cloudy till about 8^{*h*}; at which time we could only see thro' the *Huygenian* tube the second and third satellites, the first being behind *Jupiter* in the shadow, and the fourth entered upon the disk; at this time we espied a dark spot, a little northwards of the great northern zone, and near the eastern limb, where the satellite was to enter on the disk; and this spot was taken for the shadow of the satellite: The clouds again intercepted our view till 8^{*h*} 55' equat. T. at which time the first satellite was lately emerged out of the shadow, and the spot advanced so far, that we perceived it would arrive at the middle of *Jupiter*, near two hours sooner than the shadow ought to have done by our computation: but not imagining, that this dark spot could be any thing else than the shadow, we concluded

cluded there had been some error in the calculation, which we intended to re-examine afterwards. On this presumption we left off observing till 9h. 35', at which time we were surpriz'd to find a notch in *Jupiter's* limb, near the place where the former spot enter'd. This last appearance agreeing well with the time that the shadow of the satellite should have enter'd the disk, soon made us alter our former opinion, and conjecture, that this and not the other spot was the said shadow: At 9h. 39' and $\frac{1}{2}$ equat. time, the notch vanishing, a round black spot appeared within the limb, but in contact with it. At 9h. 45' we judg'd the first spot; and at 11h. 45' the second, to be in the middle of *Jupiter*.

At 11h. 50' the first spot touch'd the limb, being within the disk; soon after which the limb in that place seem'd somewhat protuberant: At 12h. 5' appear'd the fourth satellite just come out of the disk, and touching the limb in the place where the protuberance was. At 12h. 7' we could perceive the satellite separated from the limb. At 13h. 56' the second black spot, still within the disk, just touch'd the western limb; soon after which there appeared a notch in this part of the limb, as it did on the other at the coming on of this spot. At 14h. 6' the spot was quite gone off, and the limb appeared clear and entire. The first spot, when in the middle of *Jupiter* was almost as black as the second when near the limb, but somewhat less and a little more northerly.

At the time that the first spot was in the middle of the disk, the three innermost satellites appeared to the east of *Jupiter*; the first having as aforesaid, lately emerged out of the shadow; the second being almost at its greatest distance; and the third having pass'd the axis of the shadow about 12 hours before, and appearing at this time about three diameters of *Jupiter* from his limb: The times that these spots arriv'd at the middle of the disk are agreeable to the times found by calculation, in which the fourth satellite and its shadow should have appeared there. From all which it is very plain, that the first of these spots was the fourth satellite itself, and the second its shadow.

We have seen the first and second satellites appearing not as dark spots, but as bright ones (somewhat different from the light of *Jupiter*) for some little time after they enter'd his disk; but as they approach'd nearer the middle we lost sight of them; and we have frequently observ'd, that the same satellites appear brighter at some times than at others; and that when one of them has shone with its utmost splendour, the light of another

another has been considerably diminish'd: Whence at least it is very probable, not only that the satellites revolve upon their proper axes; but also that some parts of their surfaces do very faintly, if at all, reflect the solar rays to us.

All which was observ'd and taken notice of by M. Cassini and Miraldi; vide *Memoirs of the Royal Academy* for the years 1701, 1714.

An Account of the situation of the ancient Carteia and other Roman Towns near it; by Mr. Conduitt. Phil. Trans. N^o 359. P. 903.

ABOUT four English miles N. W. from Gibraltar, at the end of the bay, there are considerable ruins; the place is at present call'd *Rocadillo*, consisting of a few huts, and a modern square tower, which appears to have been rais'd on the foundation of a much larger pile: The walls of the old city may be easily traced; they seem to have been about two English miles in circumference; and built upon the brow of a rising ground; the space within is cover'd with ruins, among which are a great many pieces of very fine marble well wrought, and a vast many fragments of vessels of that kind of red earthen ware, which *Ambrosio Morales* in chap. 1. of his *Discurso de las antiquedades de las ciudades de Espanna*, lays down as a certain mark of a Roman city, and which he takes to have been a composition of the clay of *Saguntum*, often mention'd among the Romans.

Ficta Saguntino pocula malo luto. Mart. lib. 8. ep. 6.
Same Saguntino pocula ficta luto. Id. lib. 14. ep. 108.

There are the remains of a rude semi-circular building rais'd on arches, which descends gradually into an area, and seems to have been a kind of theatre: Mr. Conduitt had a marble pedestal of a statue, dug up near the square tower; the marks where the feet and the extremities of the drapery were fastened to it were still to be seen, and the following letters finely cut, *VARIA MARCE*; upon it were three other letters *LLA*, which were afterwards broken off: There were other inscriptions, but defaced and ill cut: Mr. Conduitt had a considerable number of medals found among these ruins; most of them with a *Caput turratum* and *CARTEIA* in very legible characters; the reverse was generally a *Fish*, a *Neptune* or a *Rudder*. Towards the west there is an easy descent to the river

river *Gaudaranque*, which takes its rise at *Castellar* about four leagues in the country, and is very deep at *Rocadillo*: There is a bar where the river falls into the bay; but it does not hinder the entrance of vessels of 15 tons to load charcoal and other necessaries that are ship'd off from thence for *Ceuta*: Along the side of the river there is still a great deal of stonework and visible remains of an ancient key. At a little distance to the east, upon an eminence, there are considerable ruins of a square castle, which appears to have been an ancient building of very great strength: The country people now call it *Castillon*; but the *Corrigidor* of that district told Mr. *Conduitt* he remembered it call'd *Torre Cartagena*: The situation agrees exactly with the tower of that name, mentioned in *chap. 274* and *316* of the *Chronicle of Alphonso XI. of Castile*, a book of great authority among the *Spaniards*, who are generally of opinion, that it was form'd upon the memoirs of *Fernando Nunnez de Valladolid*, a favourite and minister of that king, tho' it goes under the name of another person.

All the *Spaniards* living about the ruins, affirm them to be the remains of a city of the *Gentiles* call'd *Cartago*; the corruption of *Carteia* into a name much more famous might easily happen in an oral tradition of so many years; and Mr. *Conduitt* thinks, that where other circumstances concur, an account deliver'd down from father to son is an evidence not to be slighted in matters of so much obscurity.

Frequent mention is made of *Carteia* by the ancient Geographers and Historians; and Mr. *Conduitt* building so much on two passages of *Livy*, inserts them at length; the first is in *lib. 28. cap. 30*, to the following purpose; in the mean time *Lælius* sailing (*Livy* does not mention from what port, but by what goes before, it seems to have been from *Cartagena*, at that time *Scipio's* head quarters) thro' the streights into the ocean, came with his fleet to *Carteia*, situated on the coast of the ocean, where the sea begins to widen from the mouth of the streights; he had hopes, as was said before of taking *Cadix* by treachery, there having come of their own accord into the *Roman* camp such who promised to betray the town; but being discover'd before the plot was ripe for execution, *Mago* seiz'd the traitors and deliver'd them to *Adherbal* the *Prætor* to be carried to *Carthage*, who put them on board a *Quinqueremis*, as sailing slower, while he himself followed at some distance with eight sail of *Triremes*: By this time the *Quinqueremis* enter'd into the streights, when *Lælius*, sailing

‘ from the harbour of *Carteia* in a *Quinqueremis* with seven
 ‘ other *Tritemes*, made towards *Adherbal* and his *Tritemes*,
 ‘ well supposing, that when the *Quinqueremis* was got into the
 ‘ rapid current it could not stem it: Whilst *Adherbal* was
 ‘ somewhat at a stand how to behave on this sudden emergency,
 ‘ whether to follow the *Quinqueremis* or direct his course to-
 ‘ wards the enemy, even his deliberating put him on the neces-
 ‘ sity of fighting; for now they were within reach of the enemy,
 ‘ who charged on all hands; the current deprived them of the
 ‘ power of managing their ships, &c.’

The other passage is in *lib. 43. cap. 3.* ‘ There came an
 ‘ embassy out of *Spain* from a sort of people, who alledged
 ‘ themselves descendants of *Roman* soldiers by *Spanish* women,
 ‘ to whom they had not been married, entreating that they
 ‘ might have a town allowed them to dwell in, being in all
 ‘ upwards of 4000 in number. The senate decreed they should
 ‘ go to the Tribune *L. Canuleius* and enter their names in the
 ‘ tax-book; and that such as he manumitted, might go to
 ‘ *Carteia* on the ocean; and that such of the inhabitants as
 ‘ inclined to continue, should be reckon’d in the number, and
 ‘ have lands assigned them; that it was a *Latin* colony, and
 ‘ call’d the colony of the *Libertini*.

The best *Spanish* authors take this *Carteia* of *Livy* to be
 different from that next *Calpe*, and generally place it about
Conil: *Rodrigo Caro* in his *Convento juridico de Sevilla*, *cap. 24.*
 applies the *Carteia* in the 43 book of *Livy* to *Rocadillo*, and
 in *cap. 74.* to *Carteia* near *Lepo*: It is surprising, that he takes
 no notice of the passage in the 28th book; for, the particular
 mention of *ad oceanum*, and *Urbs ea in ora Oceani sita est*,
 implies they both relate to the same place; perhaps it was
 because he could not reconcile it with his *Carteia* near *Lepo*,
Cellarius lib. 2. cap. 1. makes *Besippo* this *Carteia* of *Livy*;
 tho’ in all the ancient geographers *Besippo* be mentioned by
 itself as a distinct town; *Mr. Conduitt* is so far from seeing any
 necessity of erecting a new *Carteia* upon the ocean for those passa-
 ges in *Livy*, that he takes that in *lib. 28.* to be rather a proof
 that the city there mentioned stood at *Rocadillo*, it certainly
 agreeing much better with that situation than with *Conil* or
Carteia near *Lepo*; it is not to be reconciled with the latter,
 because that lies north west of *Cádiz*, from whence *Adherbal*
 set out for *Carthage*, and is a good way up the country, on the
 side of a river, and not in *ora oceani*; neither can *Conil* be said
 properly to be situated, *ubi primum e faucibus angustis pandi-*

tar mare; for, the sea widens considerably, before it reaches the capes *Spartel* and *Trafalgar*, and becomes an ocean where that town stands: It is observable, that *Mela* applies words of the same import with those of *Livy* to the sea between *Calpe* and *Abila Barbesul*, *aperit deinde angustissimum pelagus*; there is no harbour at *Conil*, or any other place between cape *Trafalgar* and *Cadix*: If the *Carthaginian Quinqueremis* had only been going into (*intrabat*) the mouth of the streights between cape *Spartel* and *Trafalgar*, *Lælius* could not have believed it *facis deprehensam rapido in freto in adversum æstum reciprocari non posse*; for, there is no such strong current there; and the action between him and *Adherbal's Triremes*, which were at some distance behind the *Quinqueremis*, must have happened westward of those capes, which is inconsistent with the description *Livy* gives of it; because, in that part of the ocean there are none of those eddies that appear to have had so particular an effect on both the fleets, during the engagement, and are peculiar to the middle of the gulf.

This general mistake seems to have been occasioned by giving too easily into the opinion, that *Livy* understood by *Fretum* all the sea between the capes *Spartel* and *Trafalgar*, and the rock of *Gibraltar* and *Apes-hill*; when it is more probable, that he call'd strictly so the narrowest part only, generally reckon'd to be between the two latter: *Proxima Africa & Europa littora montes efficiunt Calpe & Abila*, says *Mela*. *Pliny* takes *Mellaria* to be nearest *Afric*, and therefore, he places there the *Fretum ex Atlantico mari lib. 3.* which is an argument his *Fretum* was not the same with our streights; and that he carried the *Atlantic* much farther east than the capes *Spartel* and *Trafalgar*.

Other authors seem to make *Hercules's* pillars the boundary of the *Mediterranean* and the ocean: *Marcianus Heracleotes*, *Ἐνταῦθα πύλας ἔχει τῆς Βασιλικῆς Ἰσπανίας τὸ μέρος τὸ παρῆκον παρ' ἑα-
τερας τὰς θαλάσσας τὰς περὶ τὸν Ἡρακλεῶν πορθμὸν τυγχάνουσαι,
τὴν τε κατ' ἡμᾶς καὶ τὴν ἑξω, τὴν ἐν τῷ Ὠκεανῷ, ἰ. ε. here ends
that part of *Hispania Betica* that borders upon both the seas
about *Hercules's* pillars, to wit, the *Mediterranean* and the ocean:
*Id. τῆς μὲν Βασιλικῆς τοῦ πλείστον πρὸς τῆς κατ' ἡμᾶς κείται θαλάσσης
τῶν Ἡρακλεῶν ἐντος ὑπὸν, μέρος δὲ τι παρὰ τὸν Ὠκεανῷ, ἰ. ε. the greatest part of *Betica* lies along the *Medi-
terranean* within *Hercules's* pillars, and a part of it borders on
the western ocean.**

Polybius lib. 3. says, Καλῆσαι δὲ τὸ μὲν παρὰ τὴν καθ' ἡμᾶς παρὴ-
 κον ὡς Ἡρακλείων στήλων, Ἰβηρία; τὸ δὲ παρὰ τὴν ἰξω καὶ μεγάλην
 προσαγορευομένην, κοινὴν μὲν ὀνομασίαν ἔχει, διὰ τὸ προσφατὸς
 κατοικτεῖσθαι; i. e. that part which extends along the *Mediterra-*
nean as far as *Hercules's* pillars is called *Iberia*; and that part
 along the ocean without the said pillars, has no particular name
 as having been but lately discovered. *Appian, lib. 2.* Εμφυλ.
 παρὰ τὰς στήλας τὰς Ἡρακλείους τὸν ὠκεανὸν ὑπερῶν, i. e. after
 passing the ocean near *Hercules's* pillars.

Florus lib. 4. c. 2. writes to this purpose; the legates *Varus*
 and *Didius* fought in the very mouth of the ocean; but the
 conflict was greater with the sea, than with the ships; for, as
 if the ocean chastised the citizens for their madness, both
 fleets were cast away; and how great must the horror be, when
 waves, storms, men, ships and arms are engaged all at once;
 add to this, the dreadfulnes of the situation, the coast of *Spain*
 lying on one hand and the coast of *Mauritania* on the other,
 the *Mediterranean* here, and there the ocean and the adjoining
 pillars of *Hercules*; so that every thing seemed to contribute
 to the horror of the engagement and storm. Here the pillars
 of *Hercules* are made the very mouth of the ocean: If you un-
 derstand the *Fretum* of *Livy* in this sense, and make it signify
 only the sea between *Calpe* and *Abila*, and the ocean to begin
 from thence westwards, the passage in *lib. 28.* is an accurate
 description of *Rocadillo*. *Lælius interim freto in oceanum eve-*
ctus ad Carteiam classe accessit; urbs ea in ora oceani sita est, ubi
primum o faucibus angustis panditur mare; and allowing *Lælius*
 to set out against *Adherbal* from thence, every circumstance
 mentioned by *Livy* may be easily accounted for: A passage in
Dion Cassius lib. 43. induces Mr. *Conduitt* to believe the vessels
 anchored in the *Guadarranque*, and that that river and not the
 bay, was properly *portus Carteie*; Οὐαρος δὲ ὑποτὸν Διδίον περὶ
 Κραντίαν ἐναυκρατήθη, καὶ ὥστε μὴ προκαταφυγῶν εἰς τὴν γῆν,
 ἀγκυρας εἰς τὸ σῶμα τὸ Διμενὸς ἄλλας πρὸς ἄλλαις προερεβέληκει,
 καὶ περὶ αὐτὰς οἱ πρῶτοι τῶν διακώλων σφας, ὥσπερ περὶ ἔρμα ἐπ-
 τοικεῖσαν, παντὶ δὲ τὸ ναυτικὸν ἀπολωλεκε, i. e. *Varus*, being de-
 feated by *Didius* in a naval engagement at *Crantia*, got to
 shore, and throwing anchors into the mouth of the harbour,
 in such a manner as to be held fast by each other, the first ships
 that pursued struck against them, as against a bar, else he en-
 dangered the loss of the whole fleet: This cannot be understood
 of the bay, because, it is three leagues over at the narrowest
 part, and much too deep for a work of such a nature, which
 might

might have been easily effected upon the bar of the river *Guadaranque*.

There is no room to doubt of the emendation *Luis Nunnes* in his *Hispanica* has made here of *Καρτῖα* instead of *Καρτῖα*; for, no ancient author mentions any other town or harbour thereabouts of a name like that; and *Carteia* was the place which held out the longest for *Pompey* the younger, and where he kept his fleets.

Florus in the passage already quoted, giving an account of the same action between *Didius* and *Varus*, represents in very lively colours, the very scene near *Rocadillo*; *adde situs ipsius formidinem, vergentia in unum hinc Hispaniæ, inde Mauritanie litora; mare intestinum & externum, imminentesque Herculis speculas, &c.*

Hirtius in the latter end of his book *de bello Hispanico* says, *Cn. Pompeius ad navale præsidium parte alterâ contendit Carteiam, quod oppidum abest à Corduba millia passuum CLXX.* which distance, as well as the circumstance of *navale præsidium*, agrees with the situation of *Rocadillo*: The ancient Geographers place *Carteia* next to *Calpe* westwards: *Pomponius Mela* after having given us a compleat picture of *Calpe* and described those lasting marks, in which so many centuries have made no alteration, says, *sinus ultra est in eoque Carteia. Strabo lib. 3.* writes of it thus, *Ἐνταῦθα δὴ ὅρος ἐστὶ τῶν Ἰβηρῶν ἢ Καλπη, ἔστι καὶ πρὸς αὐτὸ Καλπη πόλις ἐν τεσσαράκοντα σταδίοις, ἀξιολόγος καὶ παλαιὰ, ναυστάδιος ποτὲ γενομένη τῶν Ἰβηρῶν. Ἐνίοι δὲ καὶ Ἡρακλεὺς κτίσμα Λεγύσιον αὐτὸν, ὃν ἐστὶ καὶ Τιμοσθένους, ὃς φησὶ καὶ Ἡρακλείαν ὀνομαζέσθαι τὸ παλαιόν, δεικνύσαι τε μέγαν περιβολὸν καὶ νεωσοίκους; i. e. at that place stands *Calpe* a mountain in *Spain*, &c. and 40 furlongs from that is the ancient and famous town, *Calpe*, formerly a station for the ships of the *Spaniards*; the town is said to be built by *Hercules*; as among others *Timosthenes*, who says, that it was anciently called *Heraclea*, and there is shewn to this day a large compass of walls, as also the ruins of a dock for ships. *Casaubon* in his notes on this passage is of opinion it should be *Καρτῖα πόλις*; for, says he, the thing itself shews, that *Carteia* is meant; and it may be gathered hence, that as often as mention is made of it below, nothing is said of its situation any where else; but I do not find the town *Calpe* even so much as mentioned by any of the ancients.*

Marcianus Heracleotes makes *Carteia* 50 *stadia* from *Calpe*: Either of these distances agrees with *Rocadillo*, according to the part of the rock from which they reckon; for, it is upwards of six miles from *Europa-point* to *Rocadillo*.

Bochart in his *Geogr. sacræ lib. 1. cap. 24.* confirms *Casaubon's* opinion; and, says he, it is likely, that *Heraclea* was the ancient name of *Carteia*, from *Hercules* its founder; for, the *Phœnicians* called their *Hercules* Μελκαρθος. *Philo Biblius* in *Sanchoniathon* apud *Eusebium lib. 1, Præpar. ev.* γινεται Μελκαρθος ὁ καὶ Ἡρακλῆς, i. e. *Melcarthus* the same with *Hercules* was born of *Demaruntis*; And Μελκαρθος is מלך תרש Melech *Kartha* *Rox Urbis*, i. e. *Tyri*; the same with *Melcertes* or *Palemon* with the *Greeks* the god of the sea, whom they make to be the son of *Cadmus*: Hence *Hesychius* Μαλκὰ τον Ἡρακλῆα Ἀναδυσίαι; therefore it is derived from *Melcartho* or *Melech Kartha*. The town which the *Phœnician Hercules* built at *Calpe* was at first called *Melcartheia*, *Melech Karthia*, Ἡρακλεία as it were; and afterwards by *aphoresis* *Cartheia* or *Carteia*; which is frequent in the *Hebrew* compound names of places; so for instance, *Sittim* for *Abel Sittim*, *Salem* for *Jerusalem*.

Mr. Conduitt had some medals dug up at *Rocadillo*, with the head and club of *Hercules* upon them, which seem in some measure to support that great man's assertion; upon the reverse are tunny fishes, which, according to *Strabo* and *Pliny* abounded formerly near *Carteia*, and are still taken in great quantities near the shore of the east sea, at a little distance from *Rocadillo*.

Bernardo Aldrete, whom *Bochart* copies on several occasions, in *lib. 2. cap. 2.* of his *Antiquedades de Espanna* accounts for the addition of *eia* to *Cartha*, which in the *Syriac* and *Chaldaic* signifies *pulcher*, *formosus*, and was affixed to the name of this city to distinguish it from the *Cartha* in *Syria*, mentioned in *Jobua*, chap. 21. v. 34.

The *Phœnicians* seem to have founded most of the cities on this coast, and probably *Carteia* was one of their earliest settlements; for, it lies very near *Afric*, in a very inviting situation, having a bay on one side, and a river on the other that waters a rich country; its height gave it strength and a very beautiful prospect; circumstances which seem to justify *Aldrete's* interpretation of the latter part of its name.

In the *Itinerary* of *Antoninus*, it is *Calpe Carteiæ*, not *tantum due urbes diversæ*, as *Casaubon* intimates in his notes on the third book of *Strabo*; for, then it would be *Calpen Carteiæ*; nor, according to *Suirza's* comment on that part of the *Itinerary*, ut significet non rectè iter ex *Suel Carteiæ* deduci, sed paululum ad *Calpen* deflecti; because, *Calpe* stands at the end of a narrow neck of land, which projects to the southward a great way from the

the rest of the continent; and consequently, is quite out of the road from *Suel* to any other place westwards of it; probably *Calpe-Carteia* is instead of *Carteia ad Calpen*, to distinguish it from the other *Carteia* in *Celiberia*, mentioned in *Livy lib. 21. cap. 5.* for, as *Cato* observes, there is no necessity for the alteration *Sigonius* has made in that passage of *Alibea* for *Carteia*, from the text of *Polybius*, because *Livy* never mentions the other *Carteia* without adding *ad Oceanum*, *Urbs ea in ora Oceani sita est*; which distinction were needless, had there been only one city of that name. *Strabo* in his third book mentions a city called *Καρταία*, and places it near *Saguntum*, which is agreeable to the situation given this *Carteia* by *Livy*.

It seems something odd that *Mariana* and several others, should take the present *Gibraltar* to have been the ancient *Neraclea*; when neither *Pliny*, who resided so long in those parts, *Mela* who was born there, nor any ancient Geographer or historian, makes the least mention of any such city thereabouts, excepting *Strabo*; and he places it 40 *stadia* from *Calpe*, at the foot of which *Gibraltar* is situated: The *Spanish* historians give good grounds to believe, that there was no town upon that mountain, till the *Moors* invaded *Spain* under *Tariff*, who gave it the name it has retain'd ever since: The true reason of placing *Carteia* at *Tarifa* or *Algezaira*, seems to have been their not knowing any other place which agreed better with the old accounts of *Carteia*, or where the ruins of a city, which made so great a figure, could be buried; the common practice of authors, who describe places they have not seen: This appears to have been the case of most of those, especially *Mariana*, who, had he been in these parts, would not have been guilty of the oversight he has committed *lib. 16. cap. 9.* where he places two bays in the streights, one at *Gibraltar* and the other at *Tarifa*, which error he was, probably, led into by another; for, giving into the opinion, that *Tarifa* was the ancient *Carteia*, and finding that city placed in a bay by *Mela*, he concluded there must be one at *Tarifa*, which is an open road, and so much exposed, that in the least bad weather, the smallest vessels must be haul'd ashore; which circumstance alone is a sufficient proof of its not being *Carteia*, which by all accounts was a famous harbour.

Tho' there are very large ruins at *Algezaira*, they are not such as give any room to suppose them to be the remains of a *Roman* city; for, neither pieces of marble, inscriptions, or *Roman* coins are found there: The circumstance of *Varus's* shutting up the mouth of the harbour of *Carteia*, and the distance of 40 or 50 *stadia*

stidia from *Calpe*, are not applicable, either to *Tarifa* or *Algezira*; and if one of those towns was *Carteia*, to what city do those ruins belong. Since all the ancient Geographers make *Carteia* not only the nearest town to *Calpe*, but the only one in that bay: There is better reason to believe *Tarifa* stands on the ruins of another town, as shall be shewn anon.

Before Mr. *Conduitt* proceeds to describe the coast, he gives an account of some ruins he saw at *Ximena*, an inland town, about five leagues north from *Gibraltar*, situated on a rocky hill, at the bottom of which to the eastward is a very plentiful country, washed by the *Josgarganta*, a small branch of the river *Guadaro*: On the top of the hill is the old town, which, by the arches and vaults, appears to have been built by the *Moors*: On the right hand corner of the second gate, is a coarse stone with mouldings on the edges, that has the following inscription.

L. HERENNIO HE
RENNIANO
L. CORNELIUS HEREN
NIUS RUSTICUS
NEPOS EX TESTA
MENTO POSUIT
NONIS MARTIIS
SEX. QUINTILIO CON
DIANO SEX. QUIN
TILIO MAXIMO COSS.

Rodrigo Caro in his *convento juridico de Sevilla*, cap. 13. says he saw the beginning of this inscription in *Bejer de la Miel*; but when Mr. *Conduitt* was in that town, he was informed by a very intelligent person, that there is no Roman inscription in any part of it. The author of *Cadiz el Emporio del Orbe* when he inserts this inscription, makes it SEXOVINTILIO CONDIMIO; but the dash of the Q is very plain, and the other word seems rather CONDIANO. The *Latin Fasti* place Sex. *Quintilius Gordianus* & Sex. *Quintilius Maximus* Consuls in A. U. C. 903. but Dr. *Bentley* observed, that the *Greek Fasti* and *Dio* call him *Kovdiavos*, which reading is confirmed by this inscription.

Mr. *Conduitt* brought from *Seville* a piece of marble with the following words upon it.

AUC-

AUCTINUS CLEMEN
TIS SIBI
ET SUI BRITTÆ
MATER AN LX
H. S. E. SIT T. T. LEVIS

He saw another on the wall of the great church, which seems to have been the base of a statue, the inscription is as follows.

RESPUBLICA OBEN
SISE . . LO DATO
DEDI . . VIT CURAT
LIBE . . OR H . . REN
NIO RUSTICO H. M.
SINILO RESTITUTO
II VIR.

The manner in which the *Moors* have placed these inscriptions plainly shews the little value they put upon them; and there is so great a plenty of stone on the rock where *Ximena* stands, that it is not to be thought they would fetch them for such an use from any distant place; which induces Mr. *Conduitt* to believe, that a *Roman* town formerly stood there, called *OBA*.

Mr. *Conduitt* does not find any town of that name in ancient authors: *Strabo lib. 3.* mentions *Σοφοβα Μαυροβα* & *ἄλλαι πλείους*, which may possibly comprehend *Oba*. The *Geographia Nubienfis* in the fifth *Clima*, makes a town called *Rothan*, the first station from *Algezaira* to *Seville*, which, perhaps may have been this *Oba*; for, it is about a day's journey from *Algezaira*, and in the direct road from thence to *Seville*.

Mariana lib. 3. cap. 2. places the cave, where *Crassus* hid himself, near *Ximena*; the marks of it, given by *Plutarch*, are common to most others; Mr. *Conduitt* went three leagues in search of it; but the country people, having a notion that there is a treasure in it, would not shew him the way, tho' they acknowledged there were several such caves thereabouts: He further observes, that the name of the proprietor of the land where those caves are, is *Pachico*, which is nearly the same with that of the *Spaniard*, *Παχικω*, said by *Plutarch* to have entertained *Crassus* so courteously. *Hirtius* in the beginning of his

book *de Bello Hispanico* mentions a Spaniard of note, called *Patiecus*, in *Provincia Bæticâ*; quibus præfecit hominem ejus Provincia notum & non parum scientem, *L. Julium Patiecum*, which was probably the Roman name; and therefore, it is somewhat surprising, that the Latin translator of *Plutarch* should make it *Pacianus*.

Most of the ancient Geographers describe the coast westward of *Carteia*, in the following manner, *Julia Traducta*, *Mellaria*, *Bælo fluxius & oppidum*, *portus Bæsippos*, *promontorium Junonis*, &c. *Antoninus's Itinerary* makes no mention of *Julia Traducta*, and *Pliny* places it on the African coast, which *Hardouin* endeavours to account for in p. 227. of his *Nummi illustrati*. *Strabo* calls it *Juliam Jozam*, which as *Bochart* observes *lib. 1. cap. 24.* signifies the same in the Phœnician language as *Traductam* in the Latin; *Ptolemy* calls it *Transducta*; he places *Barbesula* between that and *Carteia*; but all the other ancient Geographers put both the town and river of that name eastwards of *Calpe*. Mr. *Conduitt* saw some ruins on the east side of the river *Guadiaro*, four leagues east of *Gibraltar*, which he takes to be the remains of the ancient *Barbesula*; for, he finds mention made in the *Cadiz Emporio del Orbe*, of two pieces of marble brought from thence to *Gibraltar*; on one of which was M M BARBESULANI: He was credibly informed, they were used for the fountain on the parade; the letters, probably, were either saw'd off, or turned inwards; for, they do not appear: This *Barbesula* is probably the *Barbariana*, placed in the *Itinerary X. M. P.* east from *Carteia*.

Pomponius Mela, who was born in those parts, and therefore, most to be depended on, gives the following account of the coast, according to the *Edit. Gronovii*, *sinus ultra est, in eoque Carteia, ut quidam putant, aliquando Tartessus, & quam transvecti ex Africâ Phœnices habitant; atque unde nos sumus Tingentera; Tum Mellaria & Bælo & Bæsippos usque ad Junonis promontorium oram freti occupat.* The text of *Mela* in this place has occasioned great disputes among the learned: *Casaubon* in his notes on *Strabo* says; for, I read it thus — atque unde nos sumus Tingi contraria Mellaria, aut Tingi e regione sita Mellaria; for, I am very well persuaded, says he, that *Tingi* is mentioned by *Mela* in this place; first when I consider the old reading, which is, as I have said, *Cingenteratum*, or *Tingentera*, as the learned *Elias Vinetus* finds it in his copy; so that now there can be no doubt made of it; and in the next place I find it *Mela's* usual way, to make mention of the places on the opposite

' site shore; for instance, in another place he says, *majo-
 rem Sabæi tenent partem, ostio proximam, & Carmanis contrariam*
 ' Macæ; nor is it to be regarded, that some place *Tingis* oppo-
 ' site to *Bælo* and not to *Mellaria*; since they are both so near
 ' each other, that none need be surpris'd at that.' *Salmasius*,
 whose opinion is approved by *Bochart*, makes it *Tingis altera*,
rum Mellaria, &c. and takes the preceding *transvecti* to denote
Julia Traducta. *Casaubon* seems to have been once of the same
 opinion; ' but, says he, *Ptolemy* seems to be of the same opinion
 ' with *Strabo*, who speaks of a certain town on this part of the
 ' coast of *Spain*, called *Transducta*, where those were settled,
 ' of whom *Strabo* here speaks; and of whom I formerly
 ' doubted, whether the following words of *Mela* were to be
 ' understood; *in eoque Carteia, ut quidam putant aliquando*
 ' *Tartessus, & quam transvecti ex Africâ Phœnices habitant*;
 ' for, he seem'd to speak pretty plainly of the *Transducta* of
 ' *Ptolemy* by a *Periphrasis*: Now, I am of the same opinion
 ' with those who refer it to *Carteia*.' The opinion of *Salmasius*
 seems to be the most probable; for, *Bælo* and not *Julia Traduc-
 ta* is said to be over against *Tingis*, *Mercianus Heracleotes*
 makes the two former about 250 *Stadia* distant from each other;
 and *Mellaria* is generally placed between them; therefore, they
 could not be so near each other, as *Casaubon* insinuates: Tho'
Carteia was originally founded by the *Phœnicians*, it was
 erected into a *Roman* colony long before *Mela*'s time, and there-
 fore, he could not very properly say *Carteia, quam Phœnices ha-
 bitant*; and had he intended to take notice of the founders of that
 city, it is probable, that an author, whose stile is so pure and
 accurate, would have made use of another word, or at least of
 another tense: Besides, if *Julia Traducta*, according to *Casau-
 bon*, be not meant by that passage, it must have been entirely
 omitted by *Mela*, which is very unlikely, considering he was
 born in or near it; and that it is mentioned by *Strabo*, who liv'd
 before him; and by *Ptolemy* and several others who were after
 him, and appears to have been remaining at the time the *Van-
 dals* were in possession of *Spain*; for, *Greg. Turon. lib. 2. says,*
Prosequentibus Alamannis usque ad Traductam, transito mari,
Vandali per totam Africam ac Mauritaniam sunt dispersi. The
 letters of *Tingi altera* come nearer the *Tingentera* of *Elias Vi-
 netus*, and the *Tingi Hieræ* of *Gronovius* than *Casaubon*'s *Tingi*
contraria or *Tingi e regione sita*; the *&* and the *atque*, by mak-
 ing the stop at *Tartessus* instead of *habitant*, may very well re-
 late to the same place; and it is not improbable, that *Mela* was

desirous to illustrate the obscure place of his birth by a *Periphrasis*, and a name of some *eclat*; tho' it has happened, that the method he took to do honour to it, has been the occasion, that we are in doubt even of its name.

Mr. *Conduitt* met with two medals of *Julia Traducta* among the brass *Spanish* coins; but as he cannot ascertain where they were found, he does not pretend to form from thence any judgment of the situation of the town to which they belong: But in a matter so dark he offers the following conjecture; it does not seem very improbable, that *Julia Traducta* stood, where *Tarifa* is at present: The *Spanish* authors take that town to have been built by *Tarif* at his second coming to *Spain*; Mr. *Conduitt* cannot see, what could invite him to settle on a spot, which has neither the convenience of a river, or a harbour, and is commanded by a rising ground; unless he found some houses standing, or ruins to serve for materials to build: Mr. *Conduitt* had several *Roman* coins, found there, after great rains, in the common sewer; which is some slight inducement to believe it was formerly a *Roman* town.

About a league and a half to the west of *Tarifa*, is a place, which goes now by the name of *Val de Vaca*: The country people have a tradition among them, that it was once a considerable town, but since swallowed up by the sea: There is a small brook, called *el Arroyo de Juan Francisco*, which serves to turn some mills, that a priest of that name was encouraged to build there, by finding an ancient stone-channel for the water: Mr. *Conduitt* saw some other small ruins, and was credibly assured, that there are visible remains of an old town a good way under water: There is a shoal almost off this place, that runs pretty far into the sea; perhaps *Mellaria* stood thereabouts.

Wherever it stood, its ruins must be a considerable way in the sea, if we credit *Pliny*, who in *lib. 3.* upon the testimony of a person born there, reckons only five miles from thence to *Afric*; whereas it is at present five leagues over at the narrow part: *Cassaubon* is mistaken in that note on *Strabo lib. 2.* where he says, 'but the mouth of the *Mediterranean* where narrowest is scarce 70 *stadia* broad.'

It is to be observed, that the best honey in all *Spain* is made in these parts, and that the same cause, to which the ancient *Mellaria* ow'd its name, still subsists, and has given a modern appellation to several places hereabouts, as *Playa de Orimel*, *Rio de la Miel*, *Bejer de la Miel*; the latter of these is generally reckoned by the *Spaniards* to be the old *Mellaria*, for no other reason,
but

but the name; for, it is at least two leagues from the coast of the *Sreights*; and by what Mr. *Conduitt* would judge when he was upon the spot, as near the ocean, and therefore, may as well be ascribed to the one as the other; whereas, *Mellaria*, according to all the ancient Geographers, was situated on the sea side in the *Sreights*, and reckoned by *Pliny* the nearest town to *Afric*; a plain proof, that it was not what is now *Bejer de la miel*.

About a league and a half farther west, in a small bay, there are very large ruins, which appear plainly to be the remains of a *Roman* town; a league eastwards from that place, upon an eminence, are to be seen the quarries from which the stones were fetched for building it; and all the way from thence are large remains of an aqueduct, of which there are in some places entire arches still standing: Among the ruins of the old town, Mr. *Conduitt* saw the body of a *Roman* statue of fine alabaster, somewhat bigger than the life; his guide said he had seen it entire; but as it was an idol of the *Gentiles*, they, like good *Catholics*, had broken it to pieces: He likewise told us, that urns containing old coins had been found there; the place is called *Balonia*; it is over against *Tangier*, and frequently infested by the *Moors*, on which account it is uninhabited; a small river, called *Alpariate*, runs by it; all which circumstances agree with the ancient accounts of *Belo*: Mr. *Conduitt* had a medal given him at *Tarifa*, with the following letters upon it *B A I L O*, which probably belonged to this city, called *Bailav* by *Ptolemy*. *Martianus Capella* lib. 6. mentions it under the name of *Velonienfis Betice Civitas*. The *Itinerary* of *Antoninus* places *Belo* six miles west of *Mellaria*, which is about the distance of these ruins from *Val de Vaca*.

About five leagues farther is the cape of *Trafalgar*, which *Mela* describes thus, *illud jam in occidentem & oceanum obliqua jugo excurrent, atque ei, quod Ampelusium esse dixeramus, adversum*: Near the cape's point are the ruins often mentioned by the *Spanish* authors, under the name of *Aguas de Mecca*: Mr. *Conduitt* was assur'd that there are still some ruins on the shore, and more in the sea, that run all along under the cape; particularly remains of a mole, which must have made it a tolerable harbour; these ruins seem to be the remains of old *Besippo*, *Plin.* lib. 3. *Portus Besippo*; and *Mela* writes, *Besippo usque ad Junonis promontorium oram freti occupat*.

The placing of watch-towers along the coast of *Spain* to alarm the country, upon any descent, seems to have been a practice

practice of a long standing, as appears from *Liv. lib. 22. cap. 19.*

A Letter of M. L'Abbé Conti to M. Leibnitz, dated at London, in March 1716. concerning the Dispute about the Invention of the Method of Fluxions or differential Method; with M. Leibnitz's Answer. Phil. Trans. N° 359. p. 923. Translated from the French.

M. L'Abbé Conti declines entering into a detail of particulars, as to the dispute between M. Leibnitz and Mr. Keill, or rather between the former and Sir Isaac Newton; and declares he can only historically relate what he has seen, and what he has read, and what he still wants to see and read, in order to form a proper judgment of the matter.

I have read, says the Abbé, with the greatest attention, and impartiality, the *Commercium epistolicum*, and its extract published in *Phil. Trans.* N° 342. I have seen the original letters of the *Commercium* in the custody of the Royal Society, a short letter (dated March 17. 1693, and printed at the end of Raphson's history of fluxions) written in your own hand to Sir Isaac Newton, and the old manuscript, entitled, *Analystis per series numero terminorum infinitas*, sent by Sir Isaac Newton to Dr. Barrow, and publish'd by Mr. Jones: From all which I conclude, that retrenching every thing that is foreign to the dispute, the only question seems to be, whether Sir Isaac Newton had the method of fluxions, or the infinitesimal Calculus before you, or you before Sir Isaac Newton. It is true, you were the first who published it; but you acknowledged, that Sir Isaac Newton had given considerable hints of it in the letters he wrote to Mr. Oldenburgh and others, as is shewn at great length in the *Commercium*, and its extract; what are your answers then to all this? This is what the publick still wants, in order to form an exact judgment of the affair.

Your friends expect your answer with great impatience, and they think you cannot dispense with returning an answer, if not to Mr. Keill, at least to Sir Isaac Newton himself, who bids you defiance in express terms, as you shall see by his letter.

I would fain see you at a good understanding with each other; The publick reaps no advantage from disputes, but loses irretrievably, for several ages, all the discoveries of which it is deprived by such disputes.

His

His Majesty was pleas'd to desire, I should inform him of all that pass'd between Sir *Isaac Newton* and you; I have done it to the best of my power, and I could wish it were with success for both.

Your problem was very easily resolv'd in a little time by several Geometricians, both at *London* and *Oxford*; the solution is general, extending to all sorts of curves, either geometrical or mechanical: The problem is propos'd a little equivocally; and I believe Mr. *de Moivre* is not mistaken, in saying, that we must necessarily fix the idea of a series of curves; for instance, supposing they have the same subtangent for the same *Abscissa*; this will agree not only with the conick sections, but also with a vast many other curves both geometrical and mechanical; and other suppositions may still be made for fixing this idea.

I shall take another opportunity to speak of Sir *Isaac Newton's* philosophy: We must previously agree on the method of philosophizing, and carefully distinguish between Sir *Isaac Newton's* philosophy, and the consequences several people draw from it without the least foundation; they ascribe to this great man several things, he by no means admits, as he has made appear to the *French* gentlemen, who came to *London*, on account of the great eclipse.

N. B. M. *L'Abbe Conti* also spent some hours in looking over the old letters and letter-books, kept in the archives of the Royal Society, in order to see if he could find any thing, which made either for M. *Leibnitz*, or against Sir *Isaac Newton*, and had been omitted in the *Commercium epistolicum Colini & aliorum*; but he could find nothing of that kind.

M. *Leibnitz* returned the following answer to M. *L'Abbe Conti* in a letter dated at *Hanover* the 14th of *April* 1716.

I have answer'd the letter you honour'd me with, and at the same time that Mr. *Newton* writ you, and have sent the whole to M. *Remond* at *Paris*, who will not fail to convey it to you: I have taken this way, in order to have indifferent persons and such as understand our dispute, as evidences in the affair; and M. *Remond* will also communicate it to others. I have likewise sent him a double of your letter and Sir *Isaac Newton's* letter: After which you may judge if the chicanery of some of your new friends does embarrass me much.

As to the problem, of which some among them would solve particular cases, in order to fix, as they pretend, its idea; it is likely, they will pitch upon such as are easy; for, there are
such

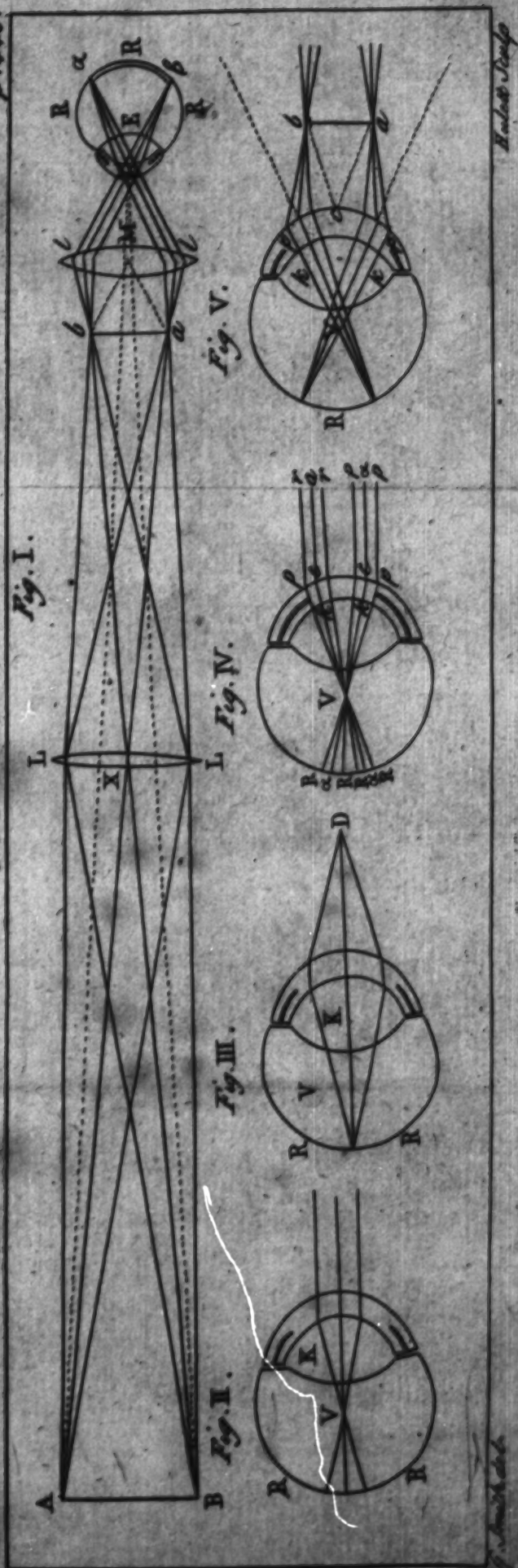
such in transcendental, as well as in common curves: But the question is to obtain a general solution: This is no new problem; M. *Jo. Bernouilli* propos'd it in the *Acta Erudit.* for the month of May 1697, p. 211. And as M. *Fatio* despis'd what we had done; the proposition was again repeated in the *Acta* for May 1700. p. 204. It may still serve to make some people sensible, whether they have made such advances as we in methods, and till they can arrive at a general solution, they may try their skill in fixing the ideas in a particular case, which I here send you: Its solution is by the same M. *Bernouilli*: And be so good as not to give in too readily to the insinuations of such as oppose us, when they would persuade you, they found no difficulty in our problem.

A problem containing a particular case of the general problem about finding a series of curves, each of which is perpendicular to another series of curves.

Upon a right line AG (Fig. 1. Plate XI.) as an axis, and from the point A having constructed any number of curves, as ABD, of such a nature, that the radius of osculi BO, drawn out from each point of each curve, for instance from B be cut by the axis AG in the point C in a constant given ratio, so that BO may be to BC, as M to N; now the trajectorial curves as ENF, are to be constructed, cutting the former curves ABD at right angles.

Thus far this letter. M. *Leibnitz* first propos'd the general problem to M. *L'Abbé Conti* in words to the following purpose; to find a line BCD, as in Fig. 2. that cuts at right angles, all the curves of a determinate series of the same kind e. g. All the hyperbola's AB, AC, AD, having the same vertex and the same centre; and that by a general method: and in the *Acta Erudit.* for October, 1698. p. 470, 471. he calls the curves in this determinate series, *curvas ordinatim datas & positione datas & positione ordinatim datas*; and by all this the series of curves to be cut is given, and nothing more is to be found than the other series which is to cut it at right angles: But M. *Leibnitz* being told, that his problem was solv'd, he changed it into a new one, of finding both the series to be cut, and the other series which is to cut it; and the particular problem, propos'd in this letter is a special case, not of the general problem first propos'd, as it ought to have been, but of this new double problem; and the first part of this double problem (*viz.* by any given property of a series of curves to find the curves) is a problem more difficult than the former

PLATE. XII.



former, and of which a general solution has not hitherto been given: M. *Leibnitz* in a letter to M. *Jo. Bernouilli*, dated December 16. 1694, and publish'd in the *Acta Erudir.* for October 1698, p. 471, set down his solution of the problem, when the given series of curves is defin'd by a finite equation, expressing the relation between the *Abscissa* and ordinate: The same solution holds, when the equation is a converging series, or when the property of the curve to be cut can be reduced to such an equation, by the *Analysis per series numero terminorum infinitas*. But M. *Leibnitz* was for solving the problem without converging series.

An Account of an ancient Roman Inscription, found at Caerleon upon Usk, by Mr. Rice; with some Conjectures thereon; by Dr. John Harris. Phil. Trans. N° 359. P. 945.

A Person being at plough in a close near the bank of the river *Usk*, call'd *Isca* by the ancients (which glides about a quarter of a mile off from *Caerleon*, and in sight of the town) came a cross a stone, and finding letters thereon, he took it up entire; it was about a yard long and about three quarters broad: The inscription is as follows.

D. M.
G. VALERIUS. G. F.
GALERIA. VICTOR
LVGDVNI. SIG. LEG. II AVG
STIP. XVII. ANNOR. XLV. CV
RA. AGINT. AMNIO. PERPETVO. B

Underneath this stone there was some seeming oblong sepulcher of stones, in no regular order; a little farther in that close, where the river wears out the land there was, some time before, a large earthen pot taken out of the bank by the river side, which contain'd the skull and bones of a human body, and which Mr. *Rice* takes to have been a *Roman* urn.

Dr. *Harris* observes, that this ancient and fair inscription confirms what others have found hereabouts, and what *Camden* and other historians relate, to wit, that the second *Roman* legion, call'd *Augusta*, brought into *Britain* by *Claudius Caesar*, under the conduct of *Vespasian*, was posted here at *Isca* or *Caer-legion* by *Julius Frontinus*, in order to awe the

Sillures, and that General obtain'd several victories over them and their neighbours in several places hereabouts.

There seems to be nothing of moment or difficulty in this inscription, but *Victor Lugduni*; which, as the Dr. thinks, we have no ground from history to refer to *Lyons in France*, so he conjectures that expression may be thus accounted for; the river *Lugg* is famous in the neighbouring parts; and as *Dynas* or *Dyn* signifies a town in the ancient *British* language; and *Dun* also signifies a hill or *Down*, as we still call it, and which the Dr. likewise takes to be deriv'd from the *British*; probably *Lugduni* here may express some town or hill near the river *Lugg*; and since there is a place, to this day call'd *Luckton*, on the side of the river *Lugg* in *Heresfordshire*, perhaps that may bid fair to be the very place, where *Valerius* obtain'd the victory, perpetuated by this inscription.

An Account of the Skeleton of a large Animal impress'd on Stone; by Dr. Stukeley. Phil. Trans. N^o 360. p. 963.

AT *Elston* near *Newark* in *Nottinghamshire*, was discover'd an almost entire skeleton (as represented in Fig. 3 Plate XI.) of a large animal, impress'd on a very hard blue clay stone, probably from the neighbouring quarries of *Fulbeck* on the western cliff of the long tract of hills, extending quite thro' the adjacent county of *Lincoln*.

The upper part and continuation of the skeleton or that which was the other side and tallied with this figure, could not be found: The skeleton was at first taken to be human, but it rather seem'd to be that of a crocodile or porpoise; there are 16 *Vertebrae* of the back and loins, very plain and distinct, with their processes and intermediate cartilages; nine whole or partial ribs of the left side; the *Os sacrum*, the *Ilium* in its natural situation, the two thigh bones displaced a little, and the beginning of the *Tibia* and *Fibula* of the right leg; on one corner there seem to be the vestiges of a foot, with four of the five toes, and a little way off an entire toe; there are no less than 11 joints of the tail, and the intermediate cartilages are of a white colour, and distinguishable from the rest: We cannot doubt but these are the real reliques of an animal; for, the very bones themselves are still to be seen as plainly, as if preserv'd in an *Egyptian* mummy: The Royal Society had a draught of a crocodile, tho' a small one, found after the like manner, inclos'd in stone; from a quarry in the mountains of
Upper

Upper Germany: The Dr. supposes the same reason accounts for both, and all the rest of these kinds of fossils, and that hence we may have a solution of some obvious and remarkable phenomena in the external face of the globe, consequent to its formation, as set forth in the *Mosaic* account; and of some changes it underwent at the universal deluge, as also proofs of that great catastrophe of the animal and vegetable world in plants, shells and parts of living creatures found in rocks and quarries.

It is remarkable, that all the stone-pits about the country whence this came, abound with vast quantities of shells and the like; and the greatest part of the substance of the stone was a composition of them: If we view a map of the country, and observe the *Lincolnshire* alps, how they run 50 miles north and south, and are steep and rocky on the west side, we may see the reason why these quarries should be so stock'd with shells; for, it is reasonable to suppose, that upon the retiring of the waters of the deluge from the superficies of this country into the eastern seas, these heavy bodies were intercepted by this cliff, which has retained such vast quantities of them ever since; whilst those that fell upon common mould are mostly rotten, and entirely lost.

We may easily conceive what petrification is from Sir *Isaac Newton's* doctrine of the attraction of the particles of matter, which is in the ratio of its solidity, proximity and surface; especially, that it is infinitely greater in the point of contact, upon which depends its cohesion, and all the varieties of physical action; and we learn, from the common experiments of chrySTALLIZATION and freezing, and even before the fire and in several chemical mixtures, how a proper degree of heat or cold, moisture, motion, rest and time, promote this principle: Whence we may know how stone gradually grow in quarries, not by any fancied vegetation, tho' there is something like it in coral, but generally by apposition of parts, as is well known in the *fluors* of subterraneous grotto's and caverns; so that we have no reason to doubt, but what was clay, sand or earth, 3000 years ago, may now be stone or marble, according as the above-mentioned causes concur: This will convince us, that the now barren and rocky plains of *Syria*, *India* and *Arabia*, are owing to natural causes, as well as to an immediate curse of God, for the disobedience of its ancient possessors; because the same is observable of the famous countries of *Greece* and *Africk*, warm regions, and so renown'd for fertility in ancient

authors: Wherefore that opinion may be true, that in many ages the whole face of the globe may become one great rock: Dr. Plott in his *Natural History of Oxfordshire*, gives an account of a *Tumulus*, now a perfect mount of stone; and upon St. Vincent's rock near *Bristol*, are fortifications, now become solid cliffs: The Dr. was shewn several bones taken from an entire human skeleton, with *British* beads, chains, iron rings, brass bits of bridles, &c. dug up in a quarry near the Earl of *Widrington's* seat at *Blankney* in *Lincolnshire*; which probably was plain mould, when these corpses of the *Britons* were interr'd; and afterwards he saw several human bones and armour, with *Roman* coins, *Fibulae*, &c. found in a stone pit in the park at *Hunstanton* in *Norfolk*, which were suppos'd to have been buried in the earth after a battle: Whence we may judge it a vulgar error, to admire the ruins of old castles and walls for the tenacity of the mortar, and to praise our ancestors for an art which we suppose now lost; when doubtless, the strength of the cement is owing to length of time; and in future ages the same judgment may be form'd of our modern buildings.

From all these instances, the Dr. infers the ancient state of these cliffs, where this skeleton was found, as also shells intimately mix'd in the substance of the stone, to have formerly been of a softer consistence, capable of admitting them into its bowels, and immuring them as part of itself; and that earth, which is now manageable by the plough, may possibly in time, assume the same density, at least not far below the surface; for, in this very cliff the upper *strata* are still clay, becoming harder the deeper you go. What sort of animal this was, cannot positively be determined for want of a natural history of skeletons; but we generally find them to be amphibious or marine animals; why such, rather than several others, should chance to be thus entomb'd, may be owing to their being able to live much longer than terrestrial animals in that collection of waters, even till they begin to abate and fall away into their destin'd receptacles; so that the bodies of the rest, soon perishing, were corrupted, and their bones separated and dispers'd much earlier; this skeleton, with others of the like kind, fell entire into the fissures of this bed of clay, which was afterwards turn'd into stone, and afforded this noble monument and pregnant proof of that general inundation.

Observations on the Strata in Coal-mines, &c. by Mr. Strachey. Phil. Trans. N^o 3612. p. 968.

MR. Strachey made the following observation on the different strata of earth and minerals found in the coal-mines of Mendip in Somersetshire: Suppose the draught represented by Fig. 4. Plate XI. to be the section of a coal-country, and to take in about four miles from the north-west to the south-east; and it may be applied to the veins of coal, as they lie at Faringdon Gourney, and likewise at Bishop-sutton, which last is near Stowey in the parish of Chew-magna in Somersetshire.

In searching for coal they first look for the *crop*, which is really coal, tho' very friable and tender, and sometimes appears to the day, as they call it; or else for the *chiff*, which is dark or blackish rock, and always keeps its regular course as the coal does, lying obliquely over it; for, all coal lies shelving like the tyle of a house, not perpendicular, nor horizontal, unless it be broken by a *ridge*, which is a parting of clay, stone or rubble; as if the veins were disjointed and broken by some violent shock, so as to let in rubble, &c. between them: The obliquity or *pitch*, as they call it, in all the works hereabout, is about 22 inches in a fathom; and when it rises to the land, it is called the *crop*, but in the north *basseting*: In the works near Stowey and likewise at Faringdon, it rises to the north-west and pitches to the south-east; but the farther they work to the south-west, the *pitch* inclines to the south; and *è contra*, when they work towards the north-east: So likewise they observe, as they work to the south-west, when they meet with a *ridge*, it causes the coal to *trap up*, that is, being cut off by the *ridge*, they find it over their heads when they are thro' a *ridge*: But on the contrary, when they work thro' a *ridge* to the north-east, they say it *traps down*, that is, they find it under their feet.

Coals is generally dug in valleys or low grounds: The surface in these parts is mostly a red soil, which, under the first or second *spitt* degenerates into *malm* or *loam*, and often yields a rock of reddish fire-stone, till you come to 4, 5, and oftentimes to 12 or 14 fathom deep, when it changes by degrees to a grey, then to a dark or blackish rock, which they call the *coal-clives*; these always lie shelving and regular as the coal does: But in these parts they never meet with *fire-stone* over the coal, as at Newcastle and in Staffordshire: These *clives* vary much in hardness, being in some places little harder than *malm* or *loam*; in others so hard

hard, that they are obliged to split them with gun-powder; and so likewise they vary in colour, the top inclining to red or grey, but the nearer they are to coal the blacker they grow; and wherever they meet with them they are sure to find coal under them; but it is not always worth the digging. The first or uppermost vein at *Sutton* is called the *stinking vein*: It is hard coal fit for mechanic uses, but of a sulphureous smell; under this about five fathoms and a half (seldom more than seven fathoms) there lies another vein, which from certain lumps of stone, mix'd with it like a *caput mortuum*, not inflammable, called *cat-head*, they call the *cathead vein*: About the same depth under this lies the *three coal vein*, so called, because it is divided into three different sorts of coal; between the first and second is a stone one foot and in some places two foot thick; but the middle and third coal seem to be placed loose on each other, without being separated by any different matter: These three veins last mentioned are sometimes worked in the same pit; but the next vein is generally worked in a separate pit; for, tho' it lies at the like depth under the other, the intermediate cliff is hard and subject to water; wherefore, he has represented a pit sunk thro' the three upper veins at A, and another sunk upon the three coal veins only at B; and so if they sink on any of the lower veins they go more to the north-west.

Next under the *three coal veins* is the *peaw vein*, so called, because the coal is figured with eyes like a peacock's tail, gilt with gold, which bird in this country dialect is called a *peaw*: The cliff over this vein is likewise variegated with cockle-shells and fern-branches; and this is always an indication of this vein, which, as is hinted above, is always searched for about 15 fathoms to the north-west of the former.

Under this again, between five and six fathoms lies the *smith's coal vein*, about a yard thick; and near the same depth under that again, lies the *shelley-vein*; and under that a vein of ten inches thick, which being little valued, has not been worked to any purpose.

Some say there is also another under the last mentioned vein, but that has not been tried within the memory of man: At *Faringdon* they have the same veins, which, as Mr. *Stracey* was informed, agree in all parts with those of *Bishop-sutton* before-mentioned; but as *Faringdon* lies four miles south-east from *Bishop-sutton*; so in the regular course they would lie a mile and a half deeper than those at *Sutton*: But as in fact they are dug near

near the same depth, it follows there must be a *trap* or several *traps* down, which in all must amount to that depth between the said works.

Between *Faringdon* and *High Littleton* the same veins seem to retain their regular course; but at *Littleton* their undermost and deepest vein is the best coal, which at *Faringdon* proves small.

On the other hand, in the parish of *Stanton-drew*, to the north-east of the coal-works at *Sutton* aforesaid, about a mile distant, and in the true course with those at *Sutton*, the same veins are found again; but here they wind a little, and their course or drift runs almost north, and they dip to the east; which winding is attributed to ridges, which the workmen have met with on both sides, and have made them discontinue the work that way: At *Stanton* they have little of the red earth or *malm* on the surface, but come immediately to an *iron-grist* or grey *tile-stone*, which is a fore-runner of the *coal-clives*; in all other things they agree with the works near *Stowey*.

In the same parish of *Stanton-drew*, a little to the eastward, they have another coal-work, but the veins are in all respects different from the former; their drift or course is to the eleven o'clock sun, as they term it, they pitch to the five o'clock morning and rise to land; consequently, to the five o'clock evening sun; They have several veins, but hitherto only three are thought worth working; the uppermost about three foot thick is small *lime-coal*; the next is about three fathoms under it and about two foot and a half thick, fit for culinary uses; the undermost is about the like depth under the former, only ten inches thick, but good hard coal.

At *Clutton* about two miles from these latter, in the same drift, viz. almost to the south-east and by south, these last veins appear again; the surface here is red, and so continues to 10, and sometimes to 14 fathoms, and in other respects they agree with the last mentioned works at *Stanton-drew*.

At *Burnet*, *Queen-Charleton*, and *Bristleton*, they have four veins which pitch to the north nearly, and consequently, the drift lies almost east and west; the surface is generally red land four or five fathoms deep; the uppermost is from three to six foot thick at *Bristleton*, but less at *Charleton* and *Burnet*: The next called *pot vein* is six fathoms under the former, 18 inches thick, all hard coal; the third is the *trench vein* seven fathoms under the other, which is from two feet and a half to three foot thick, all solid coal; the fourth is *rock vein*, always distinguished by a rock of paving stone, called *penant*, lying over it, which

which rock is sometimes 20 foot thick or more, and therefore this vein is never worked in the same pit with the former vein; but about 200 yards more to the south, or to land, as they call it; it is computed seven fathoms under the former.

The different veins of coal and earth in the coal-works in these parts, all agree in the oblique situation of the veins; and every vein has its *cliff* or *clive* lying over it, in the same oblique manner; all of them pitch or rise about 22 inches in a fathom; and almost all of them have the same *strata* of earth, *malm* and rock over them, but differ as to their course or drift, as also in thickness, goodness and use.

Now as coal is generally dug here in valleys; so the hills which interfere between the several works above-mentioned, seem also to observe a regular course in the *strata* of stone and earth found in their bowels; for, on the summits of these hills we find a stony arable soil mixed with a spongy yellowish earth and clay; under which are quarries of *lyas* in several beds, to about 8 or 10 foot deep; and six foot under that, thro' yellowish *loom*, there is a blue clay inclinable to *marle*, which is about a yard thick; under this is another whitish *loom* one yard thick, and then a blue *marle*, soft, fat and soapy, six foot thick; only at about two foot depth it is parted by a *marchasite* about six inches thick.

It is to be observed, that these beds of stone and *marle*, which are different from coal, lie all horizontal.

An Account of the great and speedy vegetation of Turnips; by Dr. Desaguliers. Phil. Trans. N^o 360. p. 274.

A Sutton-Coldfield in Warwickshire, a peaty ground near a pool (of which it was formerly a part) was sown with turnip-seed on the 2. of July 1702; in less than three days time the turnips were seen above ground; at the end of three weeks the roots were as big as walnuts; within less than five weeks after the sowing, the gardener drew great quantities of turnips, they being then as big as large apples; at the end of six weeks, viz. on the 12. of August, a large turnip was pulled up (tho' probably not so large as several others then growing on the same ground) which, together with its top and the long part of the root, weighed upwards of two pounds, 14 ounces; at the same time was also weighed an ounce of the same sort of turnip-seed; the gardener had sown his ground with, and afterwards 1000 of the grains were counted singly out of the ounce so weighed; and the rest of the ounce was divided into heaps, as near as could be guessed

guessed, equal to the 1000 seeds first severed and laid together; and it was found, that the whole ounce contained upwards of 14600 single grains, which number multiplied by 46, the number of ounces the turnip weighed, produced 671600, that is, the number of single grains of seed requisite to equal the weight of the turnip: Whence may be gathered, that (upon supposition that the increase of the turnip was all along uniform and equal, from the time it was sown, till it was pulled up) the grain of seed which it sprung from, weighing when it was sown but $\frac{1}{14600}$ of an ounce, was increased in weight according to the following proportions, viz.

In six weeks time	671600	} times its own weight.
Every { Week	111933 $\frac{1}{2}$	
Day	15990 $\frac{1}{2}$	
Hour	666 $\frac{1}{2}$	
Minute	11	

Some days after, another ounce of the same sort of seed was exactly weighed, and the grains were found to be 14673.

Another turnip of the same crop was pulled up on the 21. of October, and was found to weigh upwards of 10 pounds and a half, which unusual bulk it acquired (supposing as above the growth to be uniform) by increasing the weight of the seed it was raised from, 15 times in each minute of time, from the sowing to the drawing of it.

The gardener neglected to thin his turnips in due time, else probably their growth had been more considerable.

At another time in two other sorts of turnip seed it was found, that an ounce of one sort contained 14702 grains; and an ounce of the other sort no less than 14905 grains.

It is credibly reported, that of late years turnips have been pretty frequently found growing in several counties of England, that have weighed above twice as much, one of which was seen at Birmingham about the year 1710.

An Account of Experiments made in June with M. Villette's Burning-glass; by Dr. Harris and Dr. Desaguliers. Phil. Trans. N^o 360. p. 976.

THIS mirror is a concave 47 inches wide, and ground to a sphere of 76 inches radius; so that its focus is about 38 inches distant from the vertex of the glass; the metal of which it is made is a mixture of copper, tin and tin-glass, whose reflection

flexion has something of a yellow cast; the concave surface has scarce any flaws, and those very small; but the convex side, which is likewise polished, has some holes in it.

Having held several bodies in the *focus* of this mirror; the above gentlemen observed with a half-second *pendulum* the time, in which any material change happened to them while exposed to this great heat.

The experiments were made from 9 till 12 in the morning, as follows.

1. A red piece of a *Roman Patera* began to melt in three seconds of time, and was ready to drop in 100.

2. Another black piece melted in 4 seconds, and was ready to drop in 64.

3. Chalk, taken out of an *Echinus spatagus*, filled with chalk only, fled away in 23 seconds of time.

4. A fossile shell was calcined in 7 seconds of time and did no more in 64.

5. A piece of *Pompey's* pillar at *Alexandria* was vitrified in the black part in 50 seconds, and in the white part in 54.

6. Copper ore, that had no visible metal in it, vitrified in 8 seconds.

7. Slag or cinder of old iron-work said to have been wrought by the *Saxons*, was ready to run in 29 seconds and $\frac{1}{4}$.

Here the glass growing hot burned with much less force.

8. Iron ore fled immediately, but melted in 24 seconds.

9. Talc began to calcine at 40 seconds, and held in the *focus* 64.

10. *Calculus humanus* was calcined in 2 seconds, and only dropped off in 60.

11. An anonymous fish's tooth melted in 32 seconds and $\frac{1}{4}$.

12. The *Asbestos* seemed somewhat condensed in 28 seconds; but it was now somewhat cloudy: *M. Villette* says, that his glass usually calcines it.

13. A golden *marchasite* broke to pieces, and began to melt in about 30 seconds.

14. A silver six-pence melted in 7 seconds and $\frac{1}{4}$.

15. A king *William's* copper half-penny melted in 20 seconds, and ran with a hole in it in 31.

16. A king *George's* half-penny melted in 16 seconds, and ran in 34.

17. Tin melted in 3 seconds.

18. Cast iron in 16 seconds.

19. Slate melted in three seconds, and had a hole in 6.

20. Thin

20. Thin tyle melted in 4 seconds, had a hole and was vitrified thro' in 80.

21. Bone calcined in 4 seconds, and vitrified in 33.

An emerald was melted into a substance like a turquois stone.

A diamond weighing 4 grains lost $\frac{2}{3}$ of its weight,

An Account of an extraordinary Meteor, &c. by Dr. Halley.
Phil. Trans. N^o 360. p. 978.

ON the 19: of *March* 1718-19 there was a surprising luminous meteor seen in the heavens all over *England*: Some of its phenomena are very hard to account for, according to the notions hitherto received by our naturalists; such as, its very great height above the earth, its vast quantity of matter; the extraordinary velocity with which it mov'd, and the prodigious explosions heard at great distances, and whose sound, attended with a very sensible tremor of the subjacent air, was certainly propagated thro' a medium exceedingly rare, and next to a *vacuum*.

The Dr. formerly collected (vide *Phil. Trans.* N^o 341.) all he could find of such meteors; but none seem to come up in any circumstance to this appearance; of which he gives the following account from the several relations thereof communicated to the *Royal Society*.

Sir *Hans Sloane* being abroad at that time, happened to have his eyes turned towards it, at its very first eruption, of which he gave an account as follows; that passing along eastwards by the north east corner of *Southampton-street* in *Bloomsbury-square*, *London*, at about a quarter after eight o'clock at night, he was surpris'd to see a sudden great light, far exceeding that of the moon, which shone very bright at that time; he turned to the westward where the light was; the first place he observed it in was about the *Pleiades* northerly, whence it moved after the manner of, but more slowly than a falling star, in a seeming direct line, descending a little beyond, and withal below, the stars in *Orion's belt*, then in the south-west; the long stream appeared to him to be branched about the middle, and the meteor in its way became tapering upwards; at the lower end it at last became bigger and spherical, yet not so big as the full moon; its colour was whitish, with an eye of blue of a very vivid dazzling lustre, which seem'd very nearly to resemble if not surpass the brightness of the sun's body in a clear day, when seen with the naked eye: This brightness oblig'd him to turn his eyes several times from it, as well when it was a stream, as when it was tapering

and a globe; it seem'd in about half a minute or less, to move 20° , and to go out, as he conjectured, about as much above the horizon: It left behind it, where it had pass'd, a track of a cloudy or faint reddish yellow colour, such as red-hot iron or glowing coals have, that continued upwards of a minute, seem'd to sparkle and kept its place without falling: This track was interrupted or had a chaim towards its upper end, and about $\frac{2}{3}$ of its length: He did not hear any noise it made; but the place where the globe of light had been, continued for some time after it was extinct, of the same reddish yellow colour with the stream, and at first some sparks seem'd to issue from it, such as come from red-hot iron beaten on an anvil.

All the accounts of the phenomenon agree in this, to wit, that its splendour was little inferior to that of the sun; that within doors the candles gave no manner of light; and in the streets, that not only all the stars disappear'd, but the moon then nine days old, and high towards the meridian (the sky being very clear) was so far effaced as to be scarcely visible, at least not to cast a shadow, even where the beams of the meteor were intercepted by the houses; so that for some few seconds of time, it resembled perfect day in all respects.

The time when this happened, was generally reckoned at a quarter past eight; but by the accurate account of Mr. Pound (who only saw the light) agreeing with what has been sent us from the *Parisian* observatory, it appears to have been at 8h. 8' apparent time at *London*; and the sun being then in 9° and $\frac{1}{2}$ of *Aries*, the right ascension of the mid-heaven was $130^{\circ} 45'$, whereby the position of the sphere of fix'd stars is given: Hence the *Lucida Pleiadum* will be found at that time to have been 25° and $\frac{1}{2}$ high, in an azimuth 6° to the northward of the west; and consequently, the arch the meteor mov'd in was inclin'd to the horizon in an angle of about 27° , having its node or intersection therewith, nearly south south west, as will more plainly appear from what follows.

At *Oxford* five minutes earlier, Mr. *John Whiteside*, immediately after the extinction of the meteor, made haste out to see what it might be; and well considering the situation of the track it had left in the sky; found it to have pass'd about 1° and $\frac{1}{2}$ above the preceeding shoulder of *Orion*, and about 3° and $\frac{1}{2}$ above the middle of his belt, where there appeared a luminous *nubecula* of a reddish light, being a dilatation of the track, seeming to have been occasioned by some explosion there;

there; and by what he could learn from those who saw it, it was thereabouts it broke out, and first began to efface the stars: Hence it proceeded as to sense in an arch of a great circle, and passing in the middle between the tail of *Lepus* (Bayer's θ) and β in the fore-foot of *Canis major*, it terminated about ξ in the breast of the same, nearly in 95° R. *Ascens.* with 23° of *S. Declin.* And at the place of its extinction there remained a large whitish *nebula*, much broader and of a stronger light than the rest of the track, which he took for a certain sign of a very great explosion made there. It will be found by computation, that the angle this track made with the horizon of *Oxford* was very nearly 40° , and its intersection due S. S. W; and that the place of its extinction was about 9° above the horizon, in the azimuth of 32° to the west.

At *Worcester* Mr. *Nicholas Fatio* saw this meteor descend obliquely towards the south, making an angle with the horizon of about 65° , and intersecting it about S. S. W. $\frac{1}{4}$, as may be collected from a scheme thereof sent up by him to the Royal Society; by this the track left all *Orion* and *Canis major* to the westward, and divided the distance between *Sirius* and *Procyon*, so as to be almost twice as far from *Procyon* as *Sirius*: The time at *Worcester* was one minute before eight, this city being about 9' of time to the west of *London*; and consequently, the R. *Ascens.* of the mid-heaven 128° and $\frac{1}{4}$.

Now the situation of the cities, *London*, *Oxford* and *Worcester* being nearly on the same W. N. W. point, on which the track of the meteor had its greatest altitude above the horizon, equal to the angle of its visible way; if we suppose it at *London* to have been 27° high, and at the same time at *Worcester* to be 65° high, in the plane of the vertical circle, passing thro' *London* and *Worcester*; supposing likewise the distance between them to be 90 geographical miles, or 1° and $\frac{1}{4}$ of an arch of a great circle of the earth, we may by a trigonometrical calculation find the perpendicular height to have been 64 such miles; and the point over which it was then perpendicular to have been 30 such miles W. N. W. from *Worcester*; and the geographical mile being to the *English* statute mile as 23 to 20, this height will be no less than 73 *English* miles and $\frac{1}{4}$; and the place directly under it will be found to be about *Prestain* on the confines of *Hereford* and *Radnor-shires*: The observation at *Oxford* does likewise nearly agree in the same conclusion.

This

This altitude being added to the semi-diameter of the earth, as radius, becomes the secant of 11 degrees; so that the meteor might be seen above the horizon in all places not above 220 leagues distant therefrom: Whence it will not be surprising, that it should be seen over all parts of the islands of *Great Britain* and *Ireland*, over all *Holland*, and the hither parts of *Germany*, *France* and *Spain* at one and the same instant of time.

This suggests a considerable use, that might be made of these momentaneous appearances, for determining the geographical longitudes of places; for if in any two places, two observers, by help of pendulum-clocks, duly corrected by celestial observations, do exactly note at what hour, minute and second, such a meteor as this is kindled and extinguished, the difference of those times will be the difference of longitude of the two places; nor does it require so much as the use of a telescope, as in the methods hitherto put in practice for that purpose; so that if these appearances could be predicted, and notice given of their approach, so as to know when to expect them, the Dr. makes no difficulty of preferring this method of settling the geography of a country before all others.

Having thus fix'd one point in the line of its motion; let us now see what course the meteor took from thence; and first at the town of *Kirkby-Stephens*, on the borders of *Torkshire* and *Westmorland*, in a meridian very little to the westward of *Worcester*, but about 2° and $\frac{1}{2}$ more to the north, it was observ'd to break out as from a dusky cloud, directly under the moon, and from thence to descend, nearly in a perpendicular, almost to the horizon: Now the moon being at that time in the third degree of *Leo*, was about half an hour past the meridian: and consequently, much about a point to the west, or S. by W. and the situation of *Prestain* from *Kirkby-Stevens*, being pretty near upon the same point, it follows, that the direction of the track of the meteor was in the great circle passing over these two places.

And this is farther confirm'd by the observation of Mr. *Sam. Cruwys*, who observ'd at *Tiverton*, about 12 geographical miles nearly due north from *Exeter*, the first explosion of this meteor exactly in his zenith, as he was assur'd by applying his eye to the side of his door, which he took to be perpendicular, and looking upwards; and from thence he saw it descend to the southward directly in the same azimuth, without declining either to the right or left: Hence it is plain, that the

the track likewise pass'd over this place, which by our maps is found to lie in a line with *Prestain* and *Kirkby-Stephens*.

On this supposition, that the first explosion, attended with the reddish *nubecula*, was directly over *Tiverton*, let us compare the observation at *Oxford* with it, in order to determine more nicely the perpendicular altitude there: At *Oxford* this *nubecula* was found to be 3° and $\frac{1}{4}$ above the middle star of *Orion's* girdle at 8h. 3'; and was therefore 26° and $\frac{1}{4}$ above the horizon; and the distance between *Oxford* and *Tiverton* being $1^{\circ} 55'$ or 115 geographical miles, it will be, as the sine of $61^{\circ} 35'$ to the sine of $63^{\circ} 30'$, so is the semi-diameter of the earth, being 3437 and $\frac{1}{2}$ such miles to 3498 miles, the distance of the meteor from the center of the earth; from which deducing the semi-diameter, there remain $60 \frac{1}{2}$ geographical miles form the height of the meteor above *Tiverton*: This is confirmed by the observation of Mr. *Derham*, who at *Windsor* saw the aforesaid *nubecula* about 2° above the most southerly of the seven stars in the shield of *Orion*; that is (the time being 8h. 6') in the altitude of 23° and $\frac{1}{4}$: Whence the distance between *Tiverton* and *Windsor* being 150 measur'd, or 130 geographical miles, by a like proportion, we shall find the same height of the meteor 60 such miles, wanting only a quarter: so that in a round number we may conclude it to have been just 60 geographical, or 69 statute miles, above the earth's surface: Nor is it possible to come at a precise determination of this matter, by reason of the inaccuracy of our data, which were only the notes of persons under the surprise of the suddenness of the light, and no ways pretending to exactness: However such as they are, they abundantly evince its height to have exceeded 60 *English* miles, tho' some would have it to be no more than 38 or 40 such miles.

To compute the visible way of this meteor, either in respect of the horizon, or among the fix'd stars, and bring its motion to a *calculus*; all this may be perform'd with tolerable exactness, supposing it mov'd in the arch of a circle concentric with the earth, and but 60 geographical miles without it; and that the point of the first explosion was over the Lat. of $50^{\circ} 40'$ and $3^{\circ} 40'$ to the west of *London*; and that of the last extinction over Lat. $47^{\circ} 40'$ with $4^{\circ} 50'$ west Long. the time being fix'd to 8' past 8 o'clock at *London*: Hence it is easy, by a trigonometrical process, to obtain the visible altitude and azimuth of the meteor or either of its explosions, as seen from any place
whose

whose Long. and Lat. are known; and from the time given, the points in the sphere of stars answering to those azimuths and altitudes are readily deduced. And let such, as contend for a much less height of this meteor, try if they can, on such their supposition, to reconcile the several phenomena above-mentioned with each other, and with the observation of Mr. *Ellis* between *Gainsborough* and *Redford*; where at 8h. 5' the meteor was seen to pass precisely in the middle between *Sirius* and the fore-foot of *Canis major*, moving obliquely to the southward in a line whose direction seemed to be from the middle between the two shoulders of *Orion*; the Lat. of the place being nearly $53^{\circ} 20'$, and Long. $45'$ west from *London*; let them try how they can account for its being seen 5° high at *Aberdeen* in *Scotland*, and near as much at *Peterhead*, $\frac{1}{2}$ a degree more northerly, and then let them judge, whether it did not exceed the reputed limits of our atmosphere: Lastly, if the apparent altitude of the meteor at *Paris* was not 5° and $\frac{1}{2}$, but 11° on the W. by N. point, when it must have been in its greatest lustre, there will be no pretence to bring it lower than what the Dr. has made it, especially if it be allow'd to have followed the track he has assign'd it, viz. over *Prestain*, *Cardiff*, *Minehead* and *Tiverton* and over *Brest* in *Britany*.

Allowing this to have been the path it mov'd in, the real magnitude and velocity of this meteor might be assign'd, if the several accounts of its apparent diameter, and of the time of its passage from one of its explosions to the other, were consistent; but some of them making its visible appearance nearly equal to that of the sun, which, according to several, it far exceeded, we may suppose with the least, that at the time when it first broke out over *Tiverton*, its diameter was $\frac{1}{2}$ a degree; and its horizontal distance being 150 geographical miles from *London*, and altitude 60, the hypotenusal or real distance from the eye, will be more than 160 such miles; to which radius, the subtense of half a degree will be upwards of an *English* mile and a half, being about 2800 yards very nearly: After the same manner it is difficult to assign its velocity, whilst some make it half, others less than a quarter of a minute, in passing from its first explosion to its last extinction; but the distance it mov'd in that time being about 3° or 180 geographical miles, we may modestly compute it to have run upwards of 300 such miles in a minute; which is a velocity quite incredible, and such, that if a heavy body were projected

horizontally with the same, it would not descend by its gravity to the earth, but rather fly off, and move round its centre in a perpetual orb, resembling that of the moon.

Several of the accidents, that were reported to have attended its passage, were the effect of fancy, such as the hearing it hiss as it went along, as if it had been very near; others imagin'd that they felt the warmth of its beams; and some that they were scalded by it. But what is certain is the extraordinary noise that followed its explosion: All accounts from *Devon* and *Cornwall* and the neighbouring counties agree, that there was heard in those places the report, as it were, of a very large cannon, or rather of a broad-side at some distance, which was soon followed by a rattling noise, as if several small arms had been promiscuously discharged. What was peculiar to this sound was, that it was attended with an uncommon tremor of the air, and every where in these counties, shook the glass windows and doors of the houses very sensibly; and according to some, even the houses themselves, beyond the usual effect of cannon, tho' near; and at *Tiverton* a looking-glass that was loose in the frame, fell out on the shock and was broken: Nor was the extent of this prodigious sound known, which was heard against the easterly wind, that then blew, in the neighbourhood of *London*; and *Dr. Tabor* heard it distinctly beyond *Lewis* in *Sussex*; so that the *Dr.* cannot help thinking, but that such a meteor as this gave occasion to that famous ode in *Horace*.

Parcus Deorum cultor, &c.

Namque Diespiter

Ignе corusco nubila dividens

Plerumque per purum tonantes

Egit equos, volucremque currum,

Quo bruta tellus, &c. concutitur,

But whether the report, heard near *Lewis*, were of that explosion directly over *Devonshire*, or rather of that latter and much greater, at the extinction over *Britany*, the *Dr.* does not pretend to determine, till some farther accounts from *France*, whence hitherto, he only learned that the time of the appearance at *Paris* was at 17th past 8.

It remains to attempt something towards a solution of the uncommon phenomena of this meteor; and by comparing them with things more familiar to us, to shew at least how

they might probably be effected; And first, the unusual and continued heats of the preceding summer in these parts of the world may be supposed to have raised an extraordinary quantity of vapour of all sorts; of which, the aqueous and most others, being soon condensed by cold, and wanting a certain degree of specific gravity to buoy them up in the air, ascend but to a small height, and are quickly returned in rain, dews, &c. whereas, the inflammable sulphureous vapours, have, by an innate levity, a sort of *virescentifuga*, and not only have no need of the air to support them, but agitated by heat, will ascend in *Vacuo Boyleano*, and sublime to the top of the receiver, when most other fumes fall instantly down, and lie like water at the bottom. By this we may conceive, how the matter of the meteor might have been raised from a large tract of the earth's surface, and ascend far above the reputed limits of the atmosphere; where being disengaged from all other particles, by that principle of nature that congregates *homogenea* (as is visible in so many instances) its atoms might in length of time coalesce and run together, as we see salts shoot in water; and gradually contracting themselves into a narrower compass, might lie like a train of gun-powder in the *ether*, till catching fire by some internal ferment, as we find the damps in mines frequently do, the flame would be communicated to its continued parts, and so run on like a fired train.

This may explain how it came to move with so unconceivable a velocity; for, if a continued train of powder were no bigger than a barrel, it is not easy to say how very fast the fire would fly along it; much less can we imagine the rapidity of the accension of these more inflammable vapours, lying in a train of so vast a thickness. If this were the case, it was not a globe of fire, that ran along, but a successive kindling of new matter; and as some parts of the earth might emit these vapours in greater plenty than others, this train might in some parts thereof be much denser and bigger than in others, which might occasion several smaller explosions, as the fire ran along it, besides the great ones, which were like the blowing up of magazines. Thus we may account for the rattling noise like small arms, heard after the great peal, upon the explosion at *Ziverton*; the continuance of which for some time argues, that the sound thereof came from distances that increased.

As to the propagation of sound thro' a medium, according to the received theory of the air, above 300000 times rarer than what we breathe and next to a *vacuum*, the Dr. pretends not to determine: Hitherto the air is concluded to be the vehicle of sound; and

and in our artificial *vacuum* we find it greatly diminished: But we have this only instance of the effect of air-explosion of a mile or two in diameter, the immensity of which may perhaps compensate the extreme rarity of the medium.

An Observation of the end of the total lunar Eclipse on the 5th of March 1718, taken near the Cape of Good Hope, and serving to determine the Longitude thereof; by Dr. Halley. Phil. Trans. N° 361. p. 992.

THE missionaries sent to *China* in 1683, did by an emerſion of *Jupiter's* first ſatellite pretend to determine the longitude of the *Cape of Good Hope* to be $1^{\text{h}} 11'$, or 17° and $\frac{1}{2}$ more eaſterly than *Paris*, that is 20° from *London*; but *Dr. Halley*, in *Phil. Trans. N° 185*, concluded it could be no more than 17° ; and what ſeems to determine the controverſy, on the 5. of *March 1718*, the end of a lunar eclipse was obſerved by an officer of the ſhip *Emperor* when the viſible altitude of the moon's center was $13^{\circ} 25'$ he being then in the Lat. of $34^{\circ} 23'$ S. and as he afterwards found, juſt 180 leagues to the eaſtward of the *Cape of Good Hope*. The Dr. finds by calculation, that in that Lat. the moon had that height at $7^{\text{h}} 17'$ and $\frac{1}{2}$ p. m. and by comparing this eclipse with that obſerved with great exactneſs on *February 11, 1682* (which agrees perfectly well with his numbers) he concludes the middle of this to have been at *London* at $3^{\text{h}} 48'$ p. m. to which adding $1^{\text{h}} 46'$ for the ſemiduration (this being very certain from the obſerved continuance of the eclipse of 1682) the end will be found to have been at *London* at $5^{\text{h}} 34'$; the ſhip was, therefore, in a meridian 26° to the eaſtward of *London*; but ſhe was at that time 180 leagues to the eaſtward of the *Cape*, which diſtance in that Lat. gives 11° of Long. which deducted from the Long. of the ſhip, there remains juſt 15° or 1^{h} for the difference of meridians between *London* and the *Cape*: So that by this account the *Cape* is ſtill nearer our meridian than the Dr. had formerly made it, and near 6° nearer than *M. De la Hire* places it in his tables.

This eclipse was attended with all the circumſtances requiſite to make the concluſion as certain as the nature of the thing will admit of; for, the moon was nearly in *perigeo*, and the eclipse almoſt central; ſo that ſhe emerged out of the ſhadow as ſwiftly as poſſible; the ſea was very ſmooth, there having been little wind for upwards of 30 hours before; and the moon was not too high to be well obſerved with a foreſtaff; nor were they long at ſea before they made land; for, in leſs than five days, on the

10. of *March* at noon, they passed *Cape d'Agulhas*, the most southerly promontory of *Africa*, which then bore from them N. E. about seven leagues distant. The end of this eclipse, tho' not visible in *England*, might have been seen in *Germany*, at *Nuremberg*, *Leipsick* and *Berlin*.

Mr. *Alexander Brown* a *Scottish* gentleman, with a very good brass-quadrant upwards of two foot radius, and at the *Dutch* settlement at *Table bay*, after rectifying his pendulum-clock by correspondent altitudes, did on the 4. of *August* 1694 at 5^h 49' *mane*, observe the distance of the bright limb of the moon from the right shoulder of *Orion* to be 25° 3'; and the next morning *August* 5. at 5^h 21' 12" the same limb was distant from *Procyon* 25° 57' and at 5^h 36' 48" from the *Lucida Arietis* 58° 29'.

It were much to be wished that the moon had, either of these mornings, been accurately observed at *Greenwich* or *Paris*, or at some place in *Europe*, whose longitude from them is well known: But that failing, the Dr. had recourse to the period of the lunar motions, that is performed in 18 years and 10 or 11 days, after which the errors of the lunar computations return very nearly the same; and he found among his old observations one that tallied well with that of the 4. of *August*; viz. at *Oxford* the 23. of *July* 1676, at 13^h 11' 35" he observed the moon to apply to the star in the middle of the *Bull's* neck, marked A by *Bayer*; the star at that time was distant from the southern and nearest cusp of the moon 20' 32" by the micrometer; and at 13^h 17' 15", when it seemed to immerge upon the bright limb of the moon, it was distant from the northern cusp 23' 20"; but this was less certain by reason of the hazy air; the star was at that time in 28° 56' of *Taurus* with 1° 13' 20" N. Lat. whereby he found, that the lunar tables, founded on *Sir Isaac Newton's* correct theory of her motion, gave her place at that time only 2' too slow; which error being allowed on the 4. of *August* 1694, the result was, that 5^h 59' at the *Cape of Good Hope* was at *London* 4^h 53', whence the difference of longitude is 16° and $\frac{1}{2}$, sufficiently near what the Dr. had before determined it.

Of the Force of the Heart; by Dr. James Keill. Phil. Trans. N° 361. p. 995. Translated from the Latin.

AS the force, with which the heart throws out the blood, as estimated by *Borelli*, seemed almost incredible, so *Dr. Keill* attempted to give a solution nearer the truth; but did not in the least propose accurately to determine that force, but rather to hint at a method, by which it might probably be found out, and

to excite others more skilled in geometry to investigate a problem, the solution of which was so much wanted.

What Dr. *Jurin* principally objects to *Borelli*, Dr. *Moreland* and Dr. *Keill* is, that in estimating the force of the heart, they attempted to determine its ratio to an unactive quiescent weight, 'But since, says he, the heart itself moves in its *systole* or contraction, and puts into motion the bodies that oppose it, such as the blood and the coats of the arteries, it is evident, that the quantity of its force can be known no otherways, than by first knowing the quantity of its motion; for, motion can no more be compared with a quiescent weight than a right line, with a rectangle.' But this is far from being the case, as to either of the three Gentlemen abovementioned; tho' at the same time Dr. *Keill* sees no absurdity in the comparison; for, tho' there be no relation between the weight of a solid body and its motion; yet the moving force, if it act upon a fluid, has a certain ratio to the force of gravity; and indeed the moving force of a body, that produces a certain quantity of motion in a fluid in any given time, is equal to a weight, which falling by its force of gravity, acquires the same quantity of motion in the same time: Hence the force, with which water is thrown out at any orifice, is said to be equal to a certain weight; because the given weight, and the force that throws out the water, produce equal quantities of motion in equal times: This seems to Dr. *Keill* to be the true meaning of the *Newtonian* corollary, and which he thinks, differs not from what he has advanced on the force of the heart: Sir *Isaac Newton's* words are to this purpose, 'the force, by which the whole motion of the effluent water may be produced, is equal to a weight, &c.' which Dr. *Jurin* does not seem to have sufficiently attended to, when he says, 'that weight by which the motion of water running out of a vessel may be generated, &c.'

But Dr. *Keill* thinks if he is mistaken in this, he is so with the greatest geometricians of the age, as *Huygens* and Sir *Isaac Newton*, both of them explaining the force of fluids by the force of gravity; which the latter not only does in the above-mentioned *Corol.* but likewise in other places shews a method of finding the ratio of the resistance of a medium, that is, of the action of a fluid on a solid body, to the force of gravity or the centripetal force as may be seen in *Prop. 4, 5. lib. 2.* and their *Corollaries*: It is true the action of fluids on a solid is different from that of solids on each other: A fluid, moving with a given velocity, may sustain a given weight, since the parts of a fluid, continually succeeding each other, impinge upon the weight; consequently, the force

force of the fluid is really equal to the weight; but since this holds not as to solids, their force cannot be compared with gravity.

Further Dr. *Jurin* objects, that Dr. *Keill* makes the velocity of the blood thrown out of the heart to be equal, the whole time of the *systole*, whereas he has demonstrated it to be very unequal: This Dr. *Keill* denies; tho' whether the velocity of the blood thrown out of the heart be equal or unequal, he thinks, does not sufficiently appear; at least the latter supposition seemed to him to be better grounded.

In the next place Dr. *Jurin* objects against what is assumed by *Borelli* and other learned men, to wit, that the forces of similar muscles are in the ratio of their weights, and endeavours to establish another ratio of their forces in *Theor. 5*. But since its demonstration depends on a principle common to all his *Theorems*, and that principle is false, and does not come up to the cases to which it is applied, all that is built upon this foundation must necessarily fall to the ground; he supposes the coats of the vessels to impinge upon the blood contained within them, and by that stroke to communicate a part of their motion to the blood; and in the motion of the heart he supposes the ventricle to be a solid body, that with a given velocity impinges upon the blood, and by the stroke communicates a part of its motion to it; a supposition that neither agrees with the motion of the blood, or heart, or that of the air expired out of the lungs; and can by no reiterating of the least strokes be accommodated to their motions in such a manner, but that the conclusions thence drawn may be looked upon as uncertain and altogether false.

Since there is no intermediate space between the blood and inner surface of the heart, but both are contiguous to each other, the heart does not act upon the blood by impulse but by pressure; nor have the ventricles any degree of velocity in their initial contraction, but in time acquire velocity by contracting, as heavy bodies do by falling, or as fluids do by rarefaction, whence probably arises all the force of the heart; consequently, the motion of contraction is not equable, as Dr. *Jurin* would have it, but accelerated like the motion of a falling body; there is therefore the same difference between the impulse with which the heart according to Dr. *Jurin* strikes upon the blood, and the pressure with which it really acts, as there is between the action of a solid body in motion and the force of gravity; but these cannot be compared, as he himself

owns;

owns; consequently, the pressure or action of the heart upon the blood by impulse has not been explained by him, nor can it ever be: The very force of the heart, which he himself has discover'd, confirms this; for, if a weight, moving with a given velocity, were equal to the force of the heart, then the blood directly impell'd with all the force of the heart upon that weight, would in a moment destroy the motion of the weight; but with how great soever an impetus the blood opposes the weight it will never divest it of all its motion in an instant, consequently, the force of the heart is less than this weight, and cannot be properly express'd by the motion of a weight.

Dr. Jurin estimates the forces of fluids on solids, all along in the same manner as those of solids on each other, tho' the difference be considerable; and hence proceed all his mistakes; for, when a solid body, whose parts do firmly cohere, impinges upon another, each particle of the former does at one and the same time communicate its force to the latter; but the case is otherways in fluids, where there is no cohesion of parts, and where every part acts only in contact; therefore, when a column of water is directed upwards against a solid body, the parts of the column that are more remote from the body, communicate no force to it; besides a solid body communicates only a single impulse; but a column of water acts continually upon the body that opposes it, and the least part of the column in the least space of time makes an infinitely small impulse upon it, just in the manner that heavy bodies act by falling, to which, therefore the motion of fluids is aptly compar'd: Moreover, the whole motion of a solid body directly impinging upon another, may be destroyed in a moment; but the motion of a solid upon a fluid, diminishes only gradually, and ceases in a given time, in the same manner as gravity exerts its force on a body thrown upwards: From all which it is evident, that there is a great affinity between the force of a fluid in motion, and the force of gravity, and that the one may be fitly express'd by the other.

An Account of some Experiments relating to the Specific Gravity of Human Blood; by Dr. Jurin. Phil. Trans. N^o 361. p. 1000.

IT is well known from the experiments of 'M. Leeuwenboeck and others, that human blood consists of red globular particles, swimming in a pellucid *lymph* or *serum*; which two different substances, tho' of unequal specific gravities, yet so long

long as they continue to circulate in the veins and arteries, are prevented from separating by their motion and warmth: But when the blood comes to stagnate and cool in the porringer, the globular particles uniting together by their attractive force, and sinking by their weight, which is greater than that of the *serum*, from the *coagulum* or *crassamentum*, at the bottom of the porringer, while the *serum* swims a top.

This always is the case, when the *crassamentum* is at liberty to subside: But it often happens, that either by its adhesion to the sides of the vessel, or by the bubbles of air which the blood gathers upon falling into the porringer, and which stick to its surface, the *crassamentum* is kept from sinking, and seems to float upon the top of the *serum*.

These accidents seem to have given the first hint to that opinion, generally entertain'd by such as have writ upon this subject, namely, that the globular part of the blood is specifically lighter than the *serum*, in which it swims.

But what has so fully established this persuasion, is the authority of the excellent Mr. Boyle, who among the many valuable and curious experiments he has given us in his *Natural history of human blood*, has left the following ones upon that subject.

The specific gravity of the *serum* of human blood was found by weighing a piece of sealing wax first in *serum* and afterwards in water, to be to the specific gravity of water as 1024 to 1000.

In a second experiment which for greater accuracy was made with an instrument contriv'd on purpose, the specific gravity of *serum* was found to be to that of water as 1194 to 1000.

In a third experiment made by the same instrument, and with *serum* from the blood of another person, its specific gravity appear'd to be 1186.

The medium between these two last experiments is 1190, which has hitherto been universally receiv'd for the specific gravity of the *serum* of human blood; the first experiment being declared by Mr. Boyle himself to have been less exactly made than the other.

The specific gravity of human blood was found by Mr. Boyle to be to that of water, as 1040 to 1000; tho' on account of difficulties mentioned by him, he was far from being satisfied with this experiment, and recommended the thing to farther trials.

However, these experiments having hitherto pass'd uncontroverted, and it appearing from them, that the specific gravity

vity of *serum* was greater than that of blood in the ratio of 1190 to 1040, or of 8 to 7 nearly; it was a necessary consequence of this, that the blood-globules were specifically lighter than the *serum*, and that in a very great degree, considering the small proportion, that the bulk of the *crassamentum* was found to bearto that of the *serum*, from other experiments.

From this it was not improbably conjectured, that these globules were thin vesicles, fill'd with an aerial substance; and this opinion seem'd to receive a great confirmation, upon its being observ'd, in viewing the circulation by a microscope, that a blood globule in passing thro' a very narrow vessel, would change its figure from a globular to an oval form, and would recover its former figure, as soon as it was got thro' the narrow passage; which appearance seem'd to be naturally accounted for from the elasticity of the included *aura*.

Upon this conjecture have been built a great many solutions of the phenomena, observable in the animal economy and its disorders; the authors of all which were led into this mistake by the natural consequence of a matter of fact, for the truth of which they had so great an authority: But that the globular part of the blood is specifically heavier than the *serum*, will appear from the following experiments.

Exp. 1. The Dr. several times cut off a small part of the *crassamentum*, when by its adhesion to the sides of the porringer, it seem'd to swim upon the surface of the *serum*, and put it into another vessel fill'd with *serum*: upon which it immediately sunk to the bottom.

Exp. 2. When the *coagulum* has been buoy'd up in the *serum* by the bubbles of air adhering to its surface, he separated a small part of it, where those bubbles have been thickest, and put it into a glass of *serum*, in which it has swum as before; then setting the glass upon the air-pump, those bubbles burst one after another, as the receiver was exhausting, and the air being again admitted into the receiver, the lump of *crassamentum* sunk to the bottom of the glass.

Exp. 3. The Dr. often placed a drop of *serum* upon a clean glass before a microscope, in which he had dissolv'd a very small quantity of blood, and observ'd, that when the glass was held in a perpendicular position, the globules of blood subsided to the bottom of the drop; and inverting the glass, the globules again descended thro' the *serum* to the bottom: He had the same success with a small quantity of

serum and blood in the capillary tube: And the same thing was also observ'd by M. *Leeuwenhoeck*.

These experiments undeniably demonstrate, that the *crassamentum* or globular part of the blood is specifically heavier than the *serum*; and consequently, that it is by no means probable, that the blood globules are vesicles fill'd with air, or any other fluid lighter than *serum*: and that they are not fill'd with any sort of elastic fluid, will appear from the following experiment.

Exp. 4. In a small quantity of *serum* of human blood, the Dr. dissolv'd so much blood, as that the globules might not lie too thick together, to hinder their being seen distinctly; then having put a small drop of this liquor on the inside of a thin glass tube, he fitted the tube on to the air-pump, and placed a microscope by it, so that he could see the blood globules thro' the tube; this being done, he caus'd the tube to be exhausted, keeping his eye upon the globules all the while, in order to observe whether they dilated themselves, as the air was exhausted; but he could not perceive the least alteration, appearing exactly of the same bigness in *vacuo*, as they had done before: Whereas had they been fill'd with an elastic fluid, they would either have burst, or have been dilated to at least 70 or 80 times their former magnitude; the stop-cock being afterwards turn'd, and the air suffer'd to re-enter the tube, the blood globules still retain'd the same bigness as in *vacuo*.

In order to prevent the objections which may arise for want of experiments made in the same manner with Mr. *Boyle's*; the Dr. proceeds to demonstrate the specific gravities of *serum* and blood by hydrostatical experiments.

Exp. 5. In *November*, having let a quantity of blood stand about 24 hours in a porringer, and then drawing off the *serum* carefully with a small siphon into a convenient glass, he found by the hydrostatical balance, its specific gravity to be to that of water as 1029,8 to 1000.

Exp. 6. In *February* he examined the *serum* of a quantity of blood in the same manner, and found its specific gravity to be as 1018,6.

Exp. 7, 8, 9. In *April* he procur'd three several quantities of *serum* from the blood of different persons; the first of these was of a deep colour, inclining something to red, and a little turbid; its specific gravity was 1029, 7: The second was likewise a little turbid, and of a pale whitish colour; its specific gravity

gravity was 1030,2: The third quantity of *serum* was perfectly clear and of the colour of canary; its specific gravity was found to be 1030.

Tho' these five several experiments were all carefully made, and with a balance whose accuracy the Dr. was well assured of, yet for farther satisfaction he made the following in another manner.

Exp. 10. In *January* he drew off all the *serum*, from five or six several porringers, containing the blood of different persons; this he found to be somewhat tinged with blood, which was occasioned by his being obliged to draw it off pretty near the bottom of the porringers, in order to have a sufficient quantity for his purpose; for this reason he let it stand about two days, in which time the globular part of the blood was entirely precipitated to the bottom, and the *serum* became perfectly fine and transparent: He then drew it off with a siphon into a glass phial with a narrow neck, which he filled up to a certain mark made in the neck for that purpose; this done, he placed his phial in a nice pair of scales, in which he had a counterpoise for the weight of the phial, and found that quantity of *serum* to weigh 2284 grains and $\frac{1}{4}$: Then pouring out the *serum*, he filled the phial with common water to the same mark and found the weight of the water to be 2219 grains. From which it follows, that the specific gravity of this *serum* was 1029,4.

Exp. 11. In *July* he procured a quantity of blood taken from the temporal artery, from which he drew off the *serum* the next day, and weighing it in the same manner he found its specific gravity to be 1028,8.

These experiments agree so nearly together, that the small difference between them may very well be attributed to that which is between the *serum* of different persons; or to the variations occasioned by heat and cold in the several seasons of the year, in which they were made: So that from them we may safely determine the specific gravity of *serum* of human blood at a medium to be 1029,9, or in a round number 1030; from which the greatest variation in any of these experiments is little more than 1 in 1000; whereas the difference between Mr. Boyle's experiments and Dr. Jurin's amounts to 160 in 1000.

Exp. 12. In order to determine the specific gravity of human blood, which, by reason of its tenacity and sudden alterations upon standing, cannot be determined by the hydrostatical

balance; in *April* the Dr. took a narrow-neck'd phial, and filled it to a mark, with blood poured immediately out of the porringer, as soon as the person was blooded: He weighed this in the same manner as he had done the *serum* before, and found its specific gravity to be 1051.

Exp. 13. In *August* having filled the same phial with the blood of another person, running immediately out of the vein thro' a funnel, its specific gravity was found to be 1053.

Upon letting this stand till it was cold, he found the blood was sunk a small matter below the mark in the neck of the phial: This being filled up with water, which, in so small a quantity, could make no sensible difference from blood, he found the specific gravity of cold blood to be 1055.

Exp. 14. In *August* the last experiment being again repeated in the same manner, the specific gravity of cold blood was found 1055 as before.

Exp. 15. In *July* the arterial blood, from which the *serum* was afterwards drawn off for the 11. experiment being weighed in the same manner, its specific gravity was 1052.5.

As this arterial blood and its *serum* differ in specific gravity from venal blood and its *serum* no otherwise than the several portions of these do from each other; it is plain, that the difference in this respect between arterial and venal blood is wholly inconsiderable: We learn, it is true, from the animal œconomy, that the serous liquor is perpetually drawing off from the arterial blood by the several secretions; but as the quantity separated in one circulation is very inconsiderable, the blood must arrive in the veins nearly of the same density, as when it runs thro' the arteries.

In the 13. *Exp.* the Dr. observed, that the blood altered its specific gravity upon cooling from 1053 to 1055: Whence we may infer, that if the blood made use of in the 12. *Exp.* had been let stand till it were cold, its specific gravity would have been 1053; wherefore, taking a medium between the four last experiments, we may allow the specific gravity of cold human blood to be 1054.

The difference of 14 parts in 1000, between this and the specific gravity determined by Mr. Boyle, is easily accounted for, if we consider, that he did not make use of a vessel with a narrow neck, as plainly appears from the circumstances mentioned in his experiment; and consequently, a small error in the height of the liquor would make a considerable alteration in the specific gravity.

Since,

Since, therefore, the specific gravity of human blood is 1054, and that of its *serum* 1030, it is plain, that blood is heavier than *serum* by about 1 part in 43: From which it manifestly follows, that the globular part of the blood is specifically heavier than the *serum*, since the globular part being separated from the blood leaves the remainder or the *serum* specifically lighter than the entire mass.

But in order to determine the exact specific gravity of the blood-globules, it is first necessary to know the proportion, the whole quantity of *crassamentum*, contained in blood, bears to the *serum*: To this end Mr. Boyle has given us two several observations of the weight of the *crassamentum* and *serum*, after separating from each other in the porringer: But besides the difficulty of making this experiment with any tolerable exactness, it is to be considered, that there is a great deal of *serum* contained in the interstices of the globules, that compose the *crassamentum*.

This difficulty, however, is in some measure answered by two other experiments, which Mr. Boyle made for this purpose, after the following manner; he put a quantity of the *crassamentum*, already separated from the *serum*, into an alembic, and distilled off the remaining *serum ad siccitatem*, but without drawing off the oil, or volatile salt; after which he weighed the distilled liquors and the dry mass left behind.

By comparing these experiments with the two former, it will be found, that the entire weight of *serum*, contained in blood, is nearly $\frac{11}{14}$ of the whole; and consequently, the weight of the dried *crassamentum* is only $\frac{1}{14}$ of the blood.

But for farther satisfaction Mr. Brown an ingenious and skillful chemist made an *analysis* with a large quantity of blood, amounting to 4 pounds 14 ounces.

From this there was obtained with a very gentle heat 2 pounds, 14 ounces and 6 drachms of a phlegmatic liquor, that had scarcely that foetid scent, usual in the distillation of animal substances; and its specific gravity was nearly the same with that of common water, being but 1000,8: This being mixed with a strong solution of alum, scarce produced any *coagulum*; but exhibited a considerable one upon mixture with a solution of Roman vitriol.

The distillation being continued with the same heat, seven ounces more of phlegm were obtained considerably impregnated with volatile salt, as was manifest from the smell; the specific gravity of this was 1007, and mixing it with the best *tinctura maris*, a solution of alum and of Roman vitriol, a large
coagulum

coagulum was precipitated: In distilling these, there was lost by evaporation two ounces and two drachms.

The third portion of liquor being raised with a stronger fire, amounted to seven ounces six drachms; this was reddish and turbid, and so strongly impregnated with volatile salts, that it might very well be called a spirit; its specific gravity was 1080,1.

Besides these, there were seven drachms of volatile salt, an ounce of oil, and eight ounces four drachms of *caput mortuum*, which still retained some small remainder of the oil, as was manifest from its taking fire at the flame of a candle; in this latter part of the operation there were lost three ounces, seven drachms.

Upon making due allowance for the difference between the specific gravities of the three first portions of liquor and that of *serum*, as likewise for what was lost in the two several parts of the operation, which we may reasonably conclude to have been of a specific gravity nearly the same with that of the liquor drawn off; it will be found, that the quantity of *serum*, contained in this mass of blood, was about $\frac{1}{3}$ of the whole weight; and consequently, that the quantity of *crassamentum* was $\frac{2}{3}$ of the same weight.

If, therefore, we calculate on this supposition, *viz.* that the weight of the globular part of the blood is $\frac{1}{3}$ of the whole, we shall find the specific gravity of a blood-globule to be to that of water as 1277 to 1000.

If we follow the proportion of $\frac{1}{3}$, which results from Mr. Boyle's experiments, the specific gravity of a blood-globule will be 1242.

But this computation is in all appearance a great deal too large; for, we cannot be assured, that the whole quantity of aqueous liquor was raised from the *Serum* of the blood: On the contrary it is more than probable, that a considerable part thereof was afforded by the blood-globules themselves, especially in the latter part of the operation, when their texture must of necessity have been broken and dissolved by the strong fire, made use of: To prove this we need only consider the condition of the dried *crassamentum*, after the phlegm is drawn off, that being now a hard brittle substance; whereas, the globules in their natural state are soft and yielding: For which reasons the Dr. attempts to find the quantity of the globular part of the blood after another manner.

It appears from Mr. Boyle's observations, that the quantity of *serum*, which may be poured off from the *crassamentum*, is about

about one half of the whole mass: The remaining *crassamentum* consists of the blood-globules, and a quantity of *serum* filling up the interstices between them; which, if the globules keep their spherical form, may easily be found by the principles of common geometry, to be nearly one half of the bulk of the *crassamentum*; but if the globules by their pressure against each other change their figure the quantity of *serum* will be something less.

If this quantity of *serum*, lying between the blood-globules, be added to that poured off, it appears, that the *serum* contained in blood is about $\frac{1}{2}$ of the whole bulk; and consequently, that the blood-globules make about $\frac{1}{2}$ of the whole: Whence we shall find the specific gravity of the blood-globules to be to that of water as 1126 to 1000.

If we suppose the blood-globules to make $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$ or $\frac{1}{4}$ of the whole bulk, their specific gravity will be respectively 1174, 1150, 1102 or 1078: So that upon any of these suppositions, the specific gravity of the blood-globules will be considerably greater than that of the *serum*; and consequently, they cannot be supposed to be vesicles, filled with an aerial substance.

In order to come to a solution of this query, *viz.* what do blood-globules really consist of? It is to be observed, that blood is composed of phlegm, oil, volatile and fixed salts, and earth: For, as to the spirit, the Dr. supposes with Mr. Boyle, that it consists of the phlegm and volatile salt united together; that the *serum*, upon a chemical analysis, exhibits a great deal of the first of these, and the others in a very small quantity; that on the contrary, the *crassamentum* yields much less phlegm, but the other principles much more copiously than the *serum*.

From which *data* the Dr. thinks we may safely conclude, that the *crassamentum* or globular part of the blood consists of some phlegm united with the oil and salts, and a small quantity of earth.

But what exact proportion these several principles have to each other; what alterations are produced in the body by a change of this proportion; how and in what part these globules are formed; by what means they preserve their figure, without dissolving in the *serum*, or uniting with each other; what variations are made in their specific gravities by heat and cold; and what are the effects of those variations, are questions not very easy to be solved; and yet of so great importance to the animal œconomy, that it were to be wished, we had a number of *data* sufficient to determine them.

The

The Dr. afterwards made the following experiments, which serve to confirm the method last made use of for finding the specific gravity of the blood-globules.

In *August* he took a lump of the *crassamentum*, and wash'd it gently in fair water, to free it from the loose globules, which precipitating out of the *serum*, after the *coagulum* is form'd, do not unite into one body with it; this done, he laid it on a spongy brown paper, in order to drain off the superfluous moisture; and afterwards weighing it, first in air, and then in water, he found its specific gravity to be 1083, and $\frac{1}{4}$: Another lump of the same *crassamentum* being weigh'd in the same manner, its specific gravity was 1082,9.

In *September*, he found the specific gravity of another piece of *crassamentum* to be 1082,1: A second piece from the blood of a different person 1086,1: A third piece from the same person 1086,6.

From this it follows, that the specific gravity of the blood globules is at least 1084, which is the medium between these five experiments.

But if we allow one half of the bulk of the *crassamentum* to consist of *serum*, filling up the spaces between the blood-globules, we shall find their specific gravity 1138.

From this we must make a small abatement, because some part of the *serum* must have been squeez'd out from between the globules, by their yielding to each others pressure, when the lump of *crassamentum* lay on the paper; and this will reduce their specific gravity sufficiently near to 1126, as was determined before.

An Account of the Sunk Island in the River Humber, recover'd from the Sea; by Mr. Chamberlayne. Phil. Transf. N° 361. p. 1014.

THIS island goes by the name of the *Sunk Island*, so call'd, Mr. *Chamberlayne* supposes, from the sinking marsh-ground about it: Several old people remember when nothing of it appear'd but a waste and barren sand, and that only at low water, when for the space of a few hours it shew'd its head, and then was buried again till the retreat of the next tide; thus it successively appeared and disappeared till the year 1666, when it began to maintain its ground against the encroachment of the waves; about which time it was entirely recover'd by the care of Colonel *Gilby*, who rais'd high banks about the rising grounds, and cut deep canals for receiving and dis-

discharging the water, by which means it became firm and solid; and in a short time afforded good pasture for sheep and other cattle.

This island is about nine miles in circumference within the banks, which seem to render it impregnable against all future attacks of the sea, and is of a very fat and fertile soil, affords good grass, corn and hay, and is stocked with numerous flocks of sheep, of a larger size and finer wool than those in *Holderness*, from which it is divided by about two miles in water, and from *Lincolnshire* by about four; it is stor'd with vast numbers of rabbits; their furs are reckoned the finest in *England*, of a dark mouse-colour, shagg'd and soft as silk; there are also cows and horse constantly feeding in the place, with great plenty of wild fowl; there are only three families that live constantly upon the place.

The yearly income of the proprietor Mr. *Gilby* amounts to about 800 *l*. From the marshes of *Ottringham* there is a passage over the sands to the *Sunk* at low-water: This island lies two miles south of *Holderness*, in the river *Humber*, and four miles north of *Lincolnshire*.

A Way for Myopes to use Telescopes without Eye-glasses; by Dr. Defaguliers. Phil. Trans. N^o 361. p. 1017.

MYOPES may use telescopes without eye-glasses, an object glass alone being as useful to them, and sometimes more, than a combination of glasses.

Lemma. 1. What is requir'd of a telescope is to give large and distinct vision; that is, to make the object, as in *Galileo's* telescope, or its image, as in telescopes made up of convex lens's to appear under a great angle, and to have all the rays of those pencils, that enter the eye, meet in a point upon the retina on their respective axes.

Fig. 1. Plate XII. represents the combination of two convex lens's for the astronomical or inverting telescope; where the above-mentioned requisites are obtained; AB is the object, suppos'd at a vast distance from the object-lens LL; so that rays, coming from the extremity A of the object, will fall upon the lens LL, in the same manner as if they were parallel to their axis AX; and after passing the glass unite at a, where they project the image of the point A; from whence diverging they fall on the eye-glass ll, and having pass'd thro' it, go on parallel to each other, and enter the cornea of a common eye E, which unites those parallel rays upon its retina RRR at

a, where the image of *a* is projected: The same may be said of the rays that come from *B*; and after their several refractions thro' the two glasses, and the coats and humours of the eye, they meet upon the *retina* at *B*, where they project the distinct image of the point *b*: The rays, that come from all the points of the object *AB*, being affected after the same manner, give a distinct image of those points upon the *retina*; and therefore the object appears distinct.

The object will likewise appear magnified in the same proportion, as the angle $\angle C I =$ to $\angle M a$ (under which its image is seen) is greater than the angle $\angle A C B$, under which the object *AB* would be seen by the naked eye; as is demonstrated more at large by dioptrical writers.

Lem. 2. If parallel rays fall upon the *cornea* of a myops, or short sighted person, they will unite in the eye, before they come to the *retina*; the farther from it the more convex the eye is: But if the rays, which fall upon the *cornea*, diverge in proportion to the too great convexity of the eye, as from *D*, such rays will be refracted by the coats and humours of the eye in such a manner, as to meet in one point upon the *retina* *RR*, Fig. 2, 3, where the refraction of the rays, passing out of the chrystalline *K* into the vitreous humour *V*, is neglected, as is done in the other cases: This *Lemma* is likewise demonstrated by dioptrical writers.

Lem. 3. If two pencils of rays (in each of which all the rays are parallel to the the axis, as *a C*) fall upon different parts of the *cornea*, at the greatest distance from each other that can be allow'd for those rays to enter the pupil *PP*; their axes will, after entering the aqueous humour, converge and meet either in the vitreous or chrystalline humour, according to the convexity of the *cornea* thro' which they pass'd, and diverge again before they come to the *retina*; the rays of each pencil converging upon their respective axes, to the place where the said axes cross each other.

Demonstration. The axes *aca*, *aca*, (as in Fig. 4.) falling obliquely upon the *cornea* at *cc*, and entering from air into the aqueous humour, will be refracted towards the perpendicular to *K*; where striking more directly upon the chrystalline humour, they will go on to *aa* upon the *retina* *RR RR*, decussating at *V* within the vitreous humour; the other rays *r*, *r*, *p*, *p*, after their refraction in the aqueous humour, fall more obliquely

obliquely on the chrystalline; and therefore, are refracted again so as to meet at V, where the axes also meet, and thence go on to the retina R R R R.

Lem. 4. But if the axes of the above-mentioned pencils are parallel, the rays that accompany them diverging from a point so near the eye, that the divergency may be proportional to the too great convexity of the eye; then only the axes will meet in the eye before they come to the retina by *Lem. 3*; but the other rays will not unite upon their respective axes, till they come to the retina by *Lem. 2*.

Proposition. The Dr. supposes the eye of the short sighted person so convex, that he can see no farther than a common eye with the eye-glass of a telescope before it; then the eye of the short sighted person being in the place of the eye-glass, will receive the rays diverging from the several points of the image (projected by the object-glass in its focus) in such a manner, that they will after the several refractions meet in respective points on the retina, and the axes of the pencils that come from the extremities of the object, will in the eye make the angle $BVA = to bca$, under which the image ab is seen, by *Lem. 4*. The cornea and aqueous humour here supply the place of the eye-glass; and the chrystalline and vitreous humours that of a common eye; as represented by Fig. 5. wherein R is the retina, V the vitreous humour, and K K the chrystalline humour; and the image ba is suppos'd to be brought down from Fig. 1. which represents the astronomical telescope; the too great convexity of the eye here being in the place of an eye-glass.

It may be objected to this, that PP, the pupil of the eye being small, will take in but a very small image, or a small part of the object: But then if the eye be mov'd successively to all the parts of the space, where the eye-glass was, it can take in any part of the object; and if the object-glass be large, which may more easily be made than a large eye-glass, and the tube a foot wide or wider, as much may successively be taken in, as if an eye-glass might be had of a foot-diameter: A little practice may make any short-sighted person so ready, as to keep an object when once found, tho' the place where he stands be shaken: It would not be amiss to hold a lens in one's hand (instead of an eye-glass) to find the object at first, till custom has made it easy without it: When once the object is found, it may be easily kept.

An eye more short-sighted than the Dr. has suppos'd, will perform the office of a more convex eye-glass, being brought nearer to the distinct base of the object-glass; and an eye less convex, the office of a less convex eye-glass; but with this difference, that the more convex the eye is, the easier may any part of the object be found, and the larger and more lucid will it appear.

The Dr. has seen *Saturn's* ring very plain with an object-glass of little more than six foot radius without an eye-glass.

He also found out a way for *Presbyta* to make use of an object-glass, by placing their eye nearer the lens than its focus, by so much as their eye is flatter than a common eye, so as to make, as it were, the telescope of *Galileo*; the flat eye serving as a common eye arm'd with a concave lens; and the Dr. has fix'd the telescope in such a manner, that a *Presbyta* could read a small print at a great distance: The truth of this may be easily demonstrated.

If this experiment be made at sea with a very large tube, big enough to put in the head and move it about, and the object-glass be also large, it may not, perhaps be difficult to observe the eclipses of *Jupiter's* satellites; and this the Dr. recommends to the consideration of those that would attempt the longitude by such like observations.

New and accurate Tables for the ready computing of the Eclipses of Jupiter's first Satellite by Addition only; by Mr. Pound. Phil. Trans. N° 361. p. 1021.

IN *Phil. Trans. N° 214*, was exhibited an epitome of *M. Cassini's* curious tables for computing the eclipses of *Jupiter's* first satellite, without the help of any other numbers: The ease of this calculation gave great satisfaction to those that delight in telescopic observations, and has been of good use to encourage astronomers to ascertain the geographical longitudes of several places, by help of these eclipses, whose frequency seems to afford us the properest means for that purpose.

But it being 26 years since those tables were published, length of time has discover'd, that this satellite's motion is a small matter swifter than *M. Cassini* had suppos'd it: But *Mr. Pound* applied himself by frequent observation to rectify what he found amiss in the aforesaid calculation; and withal put it into another form, much more easy and compendious, by bringing what *M. Cassini* had given us

in odd numbers, to the milleſimals of a circle, both as to *Numb. I.* which he calls *Numb. A*, being the mean anomaly of *Jupiter* in ſuch parts; as alſo to *Numb. II.* or *Mr. Pound's Numb. B*, which is the diſtance of the mean place of *Jupiter* from the true place of the ſun; and which with the addition of the equation of *Numb. B*, gives the true angle of commutation in the ſame milleſimals of a circle; and having deducted from the *epocha's* the greateſt equation both of *Numb. A* and *B*, he reſtores them again by adding as much to the equations themſelves; by which means they all become affirmative, ſo that the whole computation is perform'd by addition only.

The reader is suppos'd to be acquainted with the method of M. *Caffini's* calculation, which is explained at large in the aforefaid N^o 214. of *Phil. Trans.*

[illegible]

The Epochs of the conjunctions of the first satellite with Jupiter.

Year	Conjunct.	No.	No.	Year	Conjunct.	No.	No.
D. H.	A.	B.	D. H.	A.	B.	A.	B.
1719	1 6 15 13	872	196	1749	0 11 9 34	400	866
1720	0 10 28 49	936	310	1750	0 01 21 11	485	789
21	1 5 2 44	40	229	51	1 10 1 5	569	998
22	0 19 14 11	175	143	52	1 0 12 33	653	612
23	0 9 28 38	209	57	53	1 8 52 37	748	531
1724	1 18 5 42	199	971	1754	0 23 4 4	822	445
1725	0 8 17 10	377	889	1755	0 13 15 32	906	359
26	1 16 57 13	462	808	56	0 3 27 0	990	273
27	1 7 8 41	546	722	57	0 12 7 3	75	191
28	0 21 20 8	630	636	58	0 2 18 30	159	110
1729	1 6 0 12	715	554	1759	1 10 58 34	243	24
1730	0 20 11 39	799	468	1760	1 1 10 1	328	938
31	0 10 23 7	883	382	61	1 9 50 35	412	856
32	0 0 34 34	967	296	62	1 0 1 2	496	770
33	0 9 14 38	52	215	63	0 14 13 0	580	684
1734	1 17 54 41	136	133	1764	0 4 24 27	665	598
1735	1 8 6 9	220	47	1765	0 13 4 13	749	517
36	0 21 17 36	305	961	66	0 3 15 58	833	431
37	1 6 57 40	389	880	67	1 11 56 2	918	349
38	0 21 9 7	473	794	68	1 2 7 29	2	263
1739	0 11 20 35	557	708	1769	1 10 47 33	86	182
1740	0 1 32 2	642	622	1770	1 0 59 0	171	96
41	0 10 12 6	726	540	71	0 15 10 28	257	10
42	0 0 23 33	810	454	72	0 5 21 56	339	924
43	1 9 3 37	895	373	73	0 14 2 0	423	842
1744	0 23 15 4	979	287	1774	0 4 13 27	508	761
1745	1 7 55 8	63	205	1775	1 12 53 31	592	675
46	0 22 6 35	148	119	76	1 3 4 58	676	589
47	0 12 18 3	232	33	77	1 11 45 1	761	507
48	0 2 29 30	316	947	78	1 1 56 28	845	421
1749	0 11 9 34	400	866	1779	0 16 7 56	929	335

The revolutions of *Jupiter's* first satellite in months.

January				N. Nu.	
D.	H.			A.	B.
1	18	28	36	0	5
3	12	57	12	1	9
5	7	25	48	1	14
7	1	54	24	2	18
8	20	23	0	2	23
10	14	51	36	2	27
12	9	20	12	3	32
14	3	48	48	3	37
15	22	17	24	4	41
17	16	46	0	4	46
19	11	14	36	4	51
21	5	43	12	5	55
23	0	11	42	5	60
24	18	40	24	6	64
26	13	8	59	6	69
28	7	37	35	7	73
30	2	6	11	7	78
31	20	34	47	7	82
February					
0	20	34	47	7	82
2	15	3	23	8	87
4	9	31	59	8	92
6	4	0	35	9	96
7	22	29	11	9	101
9	16	57	47	9	105
11	11	26	23	10	110
13	5	54	59	10	114
15	0	23	35	11	118
16	18	52	11	11	123
18	13	20	47	11	128
20	7	49	23	12	132
22	2	17	59	12	137
23	20	46	35	13	141
25	15	15	11	13	146
27	9	43	47	13	150
March.				N. Nu.	
D.	H.			A.	B.
1	24	12	23	14	155
2	22	40	59	14	159
4	17	9	35	15	164
6	11	38	10	15	168
8	6	6	46	16	173
10	0	35	22	16	177
11	19	3	58	16	182
13	13	32	34	17	186
15	8	11	19	17	190
17	2	29	46	18	195
18	20	58	22	18	199
20	15	36	58	18	204
22	9	55	34	19	208
24	4	24	10	19	213
25	22	52	46	20	217
27	17	21	22	20	221
29	11	49	58	20	225
31	6	18	34	21	230
April.					
0	6	8	34	21	234
2	0	47	10	21	239
3	19	15	46	22	243
5	13	44	22	22	244
7	8	12	58	22	248
9	2	41	34	23	252
10	21	10	10	23	257
12	15	38	46	24	261
14	10	7	22	24	265
16	4	35	58	25	270
17	23	4	33	25	274
19	17	33		25	279
21	12	1	45	26	283
23	6	30	21	26	287
25	0	48	57	27	292
26	19	27	33	27	296

The revolutions of *Jupiter's* first satellite in months.

<i>April.</i>				N. Nu.	
D.	H.	"	"	A.	B.
28	13	56	9	27	300
30	8	24	45	28	304
<i>May.</i>					
0	8	24	45	28	304
2	2	53	21	28	309
3	21	21	57	29	313
5	15	15	33	29	317
7	10	90	9	29	322
9	4	47	45	30	326
10	23	16	21	30	330
12	17	44	57	31	335
14	12	13	33	31	339
16	6	42	9	31	343
18	1	10	45	32	348
19	19	39	21	32	352
21	14	7	57	33	356
23	8	36	33	33	361
25	3	5	9	33	365
26	21	33	45	34	369
28	16	2	21	34	373
30	10	30	57	35	378
<i>June.</i>					
0	10	30	57	35	378
1	4	59	32	35	382
2	23	28	8	36	386
4	17	56	44	36	391
6	12	25	20	36	395
8	6	53	56	27	399
10	1	22	32	37	403
11	19	51	8	38	408
13	14	19	44	38	412
15	8	48	20	38	416
17	3	16	56	39	420
18	21	45	32	39	425
20	16	14	8	40	429
22	10	42	44	40	433
<i>June.</i>				N. Nu.	
D.	H.	"	"	A.	B.
24	5	11	20	40	438
25	23	39	56	41	442
27	18	8	32	41	446
29	12	37	8	42	450
<i>July.</i>					
1	7	5	44	42	455
3	1	34	20	42	459
4	20	2	56	43	463
6	14	31	32	43	468
8	9	0	8	44	472
10	3	28	44	44	476
11	21	57	20	45	480
13	16	25	55	45	485
15	10	54	31	45	489
17	5	23	7	46	493
18	23	51	43	46	498
20	18	20	19	47	502
22	12	48	55	47	506
24	7	17	31	47	510
26	1	46	7	48	515
27	20	14	43	48	519
29	14	43	19	49	523
31	9	11	55	49	528
<i>August.</i>					
0	9	11	55	49	528
2	3	40	31	49	532
3	22	9	7	50	536
5	16	37	43	50	541
7	11	6	19	51	545
9	5	34	55	51	549
11	0	3	31	51	554
12	18	32	7	52	558
14	13	0	43	52	562
16	7	29	19	53	567
18	1	57	55	53	571
19	20	26	31	54	574

The revolutions of *Jupiter's* first satellite in months.

August.				N. Nu.		October.				N. Nu.	
D.	H.	"	"	A.	B.	D.	H.	"	"	A.	B.
21	14	55	7	54	580	31	9	59	5	70	758
23	9	23	43	54	584	November.					
25	3	52	18	55	588	0	9	59	5	70	758
26	22	20	54	55	593	2	4	27	41	71	762
28	16	49	30	56	597	3	22	56	17	71	767
30	11	18	6	56	602	5	17	24	53	71	772
September.						7	11	53	29	72	776
1	5	46	42	56	606	9	6	22	5	72	781
3	0	15	18	57	610	11	0	50	41	73	785
4	18	43	54	57	615	12	19	19	17	73	790
6	13	12	30	58	619	14	13	47	53	74	794
8	7	41	6	58	624	16	8	16	29	74	799
10	2	9	42	58	628	18	2	45	5	74	804
11	20	38	18	59	632	19	21	13	40	75	808
13	15	6	54	59	637	21	15	42	16	75	813
15	9	35	30	60	641	23	10	10	52	76	817
17	4	4	6	60	646	25	4	39	28	76	822
18	22	32	42	60	650	26	23	8	4	76	827
20	17	1	18	61	655	28	17	36	40	77	831
22	11	29	54	61	659	30	12	5	16	77	836
24	5	58	30	62	663	December.					
26	0	27	6	62	668	0	12	5	16	77	836
27	18	55	42	62	672	2	6	33	52	78	840
29	13	24	18	63	677	4	1	2	28	78	845
October.						5	19	31	4	78	849
1	7	52	54	63	681	7	13	59	40	79	854
3	2	21	30	64	686	9	8	28	16	79	859
4	20	50	6	64	690	11	2	56	52	80	863
6	15	18	41	65	695	13	21	25	28	80	868
8	9	47	17	65	699	14	15	54	4	80	873
10	4	15	53	65	704	16	10	22	40	81	877
11	22	44	29	66	708	18	4	51	16	81	882
13	17	13	5	66	713	19	23	19	52	82	886
15	11	41	41	67	717	21	17	48	28	82	891
17	6	10	17	67	721	23	12	17	4	82	897
19	0	38	53	67	726	25	6	45	40	83	900
20	19	7	29	68	730	27	1	14	16	83	905
22	13	36	5	68	735	28	19	42	52	84	909
24	8	4	41	69	739	30	14	11	28	84	914
26	2	33	17	69	744						
27	21	1	53	69	749						
29	15	30	29	70	753						

The first equations of the conjunctions of the first satellite with
Jupiter.

Nu. A	Equa. Conju. add.	E. N. B.	Nu. A	Equa. Conju. add.	E. N. B.	Nu. A	Equa. Conju. add.	E. N. B.	Nu. A	Equa. Conju. add.	E. N. B.
0	39	8 15	128	12	7 26	256	0	1 31	384	11	52 26
4	38	12 16	132	11	27 26	260	0	0 31	388	12	37 26
8	37	16 16	136	10	47 26	264	0	1 31	392	13	23 25
12	36	21 16	140	10	9 27	268	0	3 31	396	14	11 25
16	35	26 17	144	9	31 27	272	0	7 31	400	14	59 25
20	34	30 17	148	8	45 27	276	0	12 31	404	15	48 24
24	33	35 17	152	8	19 27	280	0	19 31	408	16	38 24
28	32	40 18	156	7	44 28	284	0	28 30	412	17	30 24
32	31	45 18	160	7	10 28	288	0	38 30	416	18	22 23
36	30	50 19	164	6	38 28	292	0	50 30	420	19	15 23
40	29	56 19	168	6	7 28	296	1	3 30	424	20	9 23
44	29	3 19	172	5	37 28	300	1	17 30	428	21	4 22
48	28	10 20	176	5	8 29	304	1	33 30	432	22	59 22
52	27	16 20	180	4	41 29	308	1	50 30	436	22	55 22
56	26	23 20	184	4	15 29	312	2	8 30	440	23	53 21
60	25	30 21	188	3	49 29	316	2	28 30	444	24	51 21
64	24	38 21	192	3	24 29	320	2	51 30	448	25	49 21
68	23	47 21	196	3	1 29	324	3	15 29	452	26	48 20
72	22	56 22	200	2	40 30	328	3	40 29	456	27	48 20
76	22	5 22	204	2	20 30	332	4	6 29	460	28	48 19
80	21	15 22	208	2	1 30	336	4	34 29	464	29	49 19
84	20	26 23	212	1	42 30	340	5	3 29	468	30	50 19
88	19	37 23	216	1	25 30	344	5	34 29	472	31	51 18
92	18	48 23	220	1	10 30	348	6	5 28	476	32	53 18
96	18	0 24	224	0	58 30	352	6	38 28	480	33	55 17
100	17	14 24	228	0	47 30	356	7	13 28	484	34	57 17
104	16	28 24	232	0	36 30	360	7	50 28	488	35	59 17
108	15	42 24	236	0	26 30	364	8	27 27	492	37	1 16
112	14	57 25	240	0	18 30	368	9	6 27	496	38	5 16
116	14	13 25	244	0	12 31	372	9	46 27	500	39	8 15
120	13	30 25	248	0	7 31	376	10	27 27	504	40	11 15
124	12	48 26	252	0	4 31	380	11	9 26	508	41	15 14
128	12	7 26	256	0	1 31	384	11	52 26	512	42	17 14

The first equations of the conjunctions of the first satellite
with Jupiter.

Nu.	Equa.	E.		Nu.	Equa.	E.		Nu.	Equa.	E.		Nu.	Equa.	E.
A	Conj.	N.		A	Conj.	N.		A	Conj.	N.		A	Conj.	N.
	add.	B.			add.	B.			add.	B.			add.	B.
512	42 17	14		640	70 26	3		768	77 40	0		896	61 48	6
516	43 19	14		644	71 3	3		772	77 29	0		900	61 2	7
520	44 21	13		648	71 38	3		776	77 18	0		904	60 15	7
524	45 23	13		652	72 11	2		780	77 6	0		908	59 28	7
528	46 25	13		656	72 42	2		784	76 51	1		912	58 39	8
532	47 26	12		660	73 13	2		788	76 34	1		916	57 50	8
536	48 27	12		664	73 42	2		792	76 15	1		920	57 1	8
540	49 28	11		668	74 10	2		796	75 56	1		924	56 11	9
544	50 28	11		672	74 36	1		800	75 36	1		928	55 20	9
548	51 28	11		676	75 1	1		804	75 15	1		932	54 29	9
552	52 27	10		680	75 25	1		808	74 52	1		936	53 38	10
556	53 25	10		684	75 48	1		812	74 27	1		940	52 46	10
560	53 23	9		688	76 8	1		816	74 1	2		944	51 53	10
564	55 21	9		692	76 26	1		820	73 35	2		948	51 0	11
568	56 17	9		696	76 43	0		824	73 8	2		952	50 6	11
572	57 12	8		700	76 59	0		828	72 39	2		956	49 13	11
576	58 7	8		704	77 13	0		832	72 9	2		960	48 20	12
580	59 1	8		708	77 26	0		836	71 38	3		964	47 26	12
584	59 54	7		712	77 38	0		840	71 6	3		968	46 31	12
588	60 46	7		716	77 48	0		844	70 32	3		972	45 36	13
592	61 38	6		720	77 57	0		848	69 57	3		976	44 41	13
596	62 28	6		724	78 4	0		852	69 21	3		980	43 46	13
600	63 17	6		728	78 9	0		856	68 45	4		984	42 50	14
604	64 5	5		732	78 13	0		860	68 7	4		988	41 55	14
608	64 53	5		736	78 15	0		864	67 29	4		992	41 0	14
612	65 39	5		740	78 16	0		868	66 49	4		996	40 4	15
616	66 24	5		744	78 15	0		872	66 9	5		1000	39 8	15
620	67 7	4		748	78 12	0		876	65 28	5		1004	38 12	16
624	67 49	4		752	78 9	0		880	64 46	5		1008	37 16	16
628	68 30	4		756	78 4	0		884	64 3	5		1012	36 21	16
632	69 10	4		760	77 58	0		888	63 19	6		1016	35 26	17
636	69 49	3		764	77 50	0		892	62 34	6		1020	34 30	17
640	70 26	3		768	77 40	0		896	61 48	6		1024	33 35	17

The second equations of the conjunctions of the first satellite with
Jupiter.

To be added,

Nu. B. equ.	0	100	200	300	400	500	600	700	800	900
	Equat.	Equat.	Equ.	Equ.	Equ.	Equ.	Equ.	Equ.	Equat.	Equat.
0	14 0	12 52	9 45	5 30	1 37	0 0	1 37	5 30	9 45	12 52
4	14 0	12 46	9 36	5 20	1 30	0 0	1 44	5 40	9 54	12 58
8	13 59	12 40	9 26	5 9	1 23	0 1	1 52	5 51	10 3	13 2
12	13 59	12 35	9 17	4 59	1 16	0 2	1 59	6 1	10 12	13 7
16	13 58	12 29	9 7	4 48	1 9	0 3	2 7	6 11	10 21	13 11
20	13 57	12 23	8 58	4 38	1 3	0 4	2 15	6 22	10 31	13 16
24	13 56	12 17	8 48	4 28	0 57	0 5	2 24	6 33	10 40	13 20
28	13 54	12 11	8 38	4 18	0 52	0 7	2 32	6 44	10 49	13 25
32	13 53	12 4	8 28	4 8	0 46	0 10	2 41	6 55	10 57	13 29
36	13 51	11 56	8 17	3 58	0 40	0 13	2 50	7 5	11 3	13 33
40	13 49	11 49	8 7	3 58	0 35	0 16	2 59	7 16	11 13	13 36
44	13 47	11 42	7 57	3 38	0 31	0 19	3 9	7 26	11 20	13 38
48	13 44	11 34	7 47	3 29	0 27	0 23	3 19	7 36	11 27	13 41
52	13 41	11 27	7 36	3 19	0 23	0 27	3 29	7 47	11 34	13 44
56	13 38	11 20	7 26	3 9	0 19	0 31	3 38	7 57	11 42	13 47
60	13 36	11 13	7 16	2 59	0 16	0 35	3 48	8 7	11 49	13 49
64	13 33	11 5	7 5	2 50	0 13	0 40	3 58	8 17	11 56	13 51
68	13 29	10 57	6 55	2 41	0 10	0 46	4 8	8 28	12 4	13 53
72	13 25	10 49	6 44	2 32	0 7	0 52	4 18	8 38	12 11	13 54
76	13 20	10 40	6 33	2 24	0 5	0 57	4 28	8 48	12 17	13 56
80	13 16	10 31	6 22	2 15	0 4	1 3	4 38	8 58	12 23	13 57
84	13 11	10 21	6 11	2 7	0 3	1 9	4 48	9 7	12 29	13 58
88	13 7	10 12	6 1	1 59	0 2	1 16	4 59	9 17	12 35	13 59
92	13 2	10 3	5 51	1 52	0 1	1 23	5 9	9 26	12 40	13 59
96	12 58	9 54	5 40	1 44	0 0	1 30	5 20	9 36	12 46	14 0
100	12 52	9 45	5 30	1 37	0 0	1 37	5 30	9 45	12 52	14 0

The 3d equa-
tions to be ad-
ded.

The semidurations of the eclipses of *Jupiter's*
first fatellite.

Nu. A.	Equa	Nu. A.	Nu. A.	Semidu- rations. H., ..	Nu. A.	Semidu- rations. H., ..	Nu. A.	Semidu- rations. H., ..	Nu. A.	Semidu- rations. H., ..
0	3 30	1000	0	1 5 9	250	1 7 0	500	1 5 9	750	1 7 46
20	3 29	980	10	1 4 56	260	1 7 15	510	1 4 53	760	1 7 57
40	3 28	960	20	1 4 44	270	1 7 31	520	1 4 39	770	1 8 7
60	3 25	940	30	1 4 33	280	1 7 45	530	1 4 26	780	1 8 15
80	3 19	920	40	1 4 23	290	1 7 57	540	1 4 15	790	1 8 22
100	3 12	900	50	1 4 13	300	1 8 7	550	1 4 7	800	1 8 26
120	3 4	880	60	1 4 7	310	1 8 15	560	1 4 3	810	1 8 28
140	2 56	860	70	1 4 4	320	1 8 22	570	1 4 1	820	1 8 30
160	2 46	840	80	1 4 2	330	1 8 27	580	1 4 0	830	1 8 28
180	2 34	820	90	1 4 0	340	1 8 28	590	1 4 3	840	1 8 26
200	2 22	800	100	1 4 2	350	1 8 29	600	1 4 7	850	1 8 22
220	2 10	780	110	1 4 3	360	1 8 27	610	1 4 13	860	1 8 16
240	1 57	760	120	1 4 6	370	1 8 24	620	1 4 23	870	1 8 8
260	1 44	740	130	1 4 12	380	1 8 17	630	1 4 35	880	1 8 0
280	1 30	720	140	1 4 21	390	1 8 9	640	1 4 49	890	1 7 50
300	1 17	700	150	1 4 31	400	1 7 58	650	1 5 4	900	1 7 37
320	1 5	680	160	1 4 42	410	1 7 46	660	1 5 19	910	1 7 22
340	0 53	660	170	1 4 55	420	1 7 31	670	1 5 36	920	1 7 8
360	0 41	640	180	1 5 9	430	1 7 14	680	1 5 54	930	1 6 55
380	0 31	620	190	1 5 23	440	1 6 58	690	1 6 10	940	1 6 40
400	0 22	600	200	1 5 39	450	1 6 40	700	1 6 28	950	1 6 23
420	0 14	580	210	1 5 55	460	1 6 20	710	1 6 46	960	1 6 8
440	0 8	560	220	1 6 11	470	1 6 2	720	1 7 2	970	1 5 54
460	0 4	540	230	1 6 26	480	1 5 45	730	7 7 17	980	1 5 37
480	0 2	520	240	1 6 43	490	1 5 26	740	1 7 33	990	1 5 22
500	0 0	500	250	1 7 0	500	1 5 9	750	1 7 41	1000	1 5 5

The

	D. H. ; "				Num. A.	Num. B.
1719.	1	6	11	13	872	396
Novemb.	7	11	53	29	72	776
Mean Conj.	8	18	4	42	944	172
equat. 1			51	53		10 eq. B.
equat. 2			10	26		
equat. 3			3	26		182 eq. B.
	8	19	10	27		
			1	6	33	semidur. substra
Nov.	8	18	3	54		

So that by this calculation, the immersion of this satellite into Jupiter's shadow was on the 9. of Nov. at 4' after 8 in the morning equal time:

Ex. To find the time of the emersion of the first satellite out of Jupiter's shadow April 5. 1720.

	D. H. ; "				Num. A.	Num. B.
1720	0	20	22	40	956	310
April	4	13	44	22	Bifs. 22	244
Mean Conj.	5	10	7	2	978	554
equat. 1			44	13		13 eq. B.
equat. 2			0	45		
equat. 3			3	29		567 B. eq.
			1	5	40	semidur. add.
April	5	12	1	9		

Hence it appears, that the emersion required was to happen at 1' after midnight following the 5. of April equal time.

By several years observation it is discovered, that the second inequality of this satellite is owing to the progressive propagation of light, and common to all the rest of the satellites, light being found to proceed in about 7' of time from the sun to the earth; and whether with an equable motion or otherways is still a question: For this reason a third equation is added, whereby to account for the greater distance of Jupiter from the earth in *Apphelio* than in *Perihelio*; as the second equation answers to the greater

greater distance of the planet, when near the sun's conjunction, than when near his opposition.

An Account of several Experiments and Observations on Silk-worms, and their Silk in England; by Mr. Barham. Phil. Trans. N^o 362. p. 1036.

THESE experiments were made in *Chelsea* park, in the months of *May*, *June* and *July* 1719.

April 27. Mr. Barham received a small parcel of silk-worms eggs from *Languedoc*.

May 6. Early in the morning he found them hatched of themselves, the wind shifting in the night from east northerly to west southerly, changing the air of a sudden to warm, two days before the change of the moon.

After feeding and managing them according to art, thro' the whole course of their four sicknesses, they were arrived to their state of perfection, being then as thick as a man's little finger, and from four to five inches long, of a yellowish colour; and when held against the light they appeared as transparent as an egg, being of the same colour and consistence, and filled with the matter that makes the silk, which is a certain sign, that they will begin to spin in 24 hours or less; they then forsake their food (being very voracious before) and look out a convenient place to fix their first *bold-fasts*, for supporting the balls or cones they are to make, which they do in a very surprising and mathematical manner, with the help of a gummy substance that ties all together; and when the loose furzy substance is taken off and some of the silk wound off, the remainder is so smooth and compact, shining like satin, that they are made use of for artificial flowers, and esteemed the best of any thing hitherto known for that purpose: Mr. Barham weighed several hundreds of these silk-balls or cones, and found them to weigh from 39 to 40 grains with their *aurelia's* or *chrysalis* within them.

June 27. They began to spin, having been hatched 7 weeks and 3 days; and in 4 or 5 days they finished their laborious and curious work; but their balls were not fit to be removed till 8 or 10 days more.

Mr. Barham found, that an ounce of silk-balls would make about a drachm of fine silk; but to be more certain, he weighed 12 pounds of silk-balls at 4 several times, and told the balls in every 3 pounds as follows.

The first 3 pounds contained 812 balls

The second 3 pounds 842

The third 3 pounds 797

The fourth 3 pounds 868

So that the whole 12 pounds contained 3319 balls.

Which when wound off was found to yield 17 ounces of fine silk, and about 7 ounces of coarse refuse unwound; in all 1 pound and $\frac{1}{4}$ *averdupois* weight, or two pounds *Troy*; which is as great or greater yielding than in any part of the world, and the silk as fine: A silk broker took it for *Italian* silk (not knowing it was made in *England*) and said it was worth 20 shillings per pound.

Now upon this experiment finding, that 3319 silk-balls would make 1 pound and 1 ounce of fine silk; in order to know what quantity of silk might be expected from the worms hatched from 1 ounce of eggs, Mr. *Barham* made use of the following method: He found by often weighing and telling, that 100 eggs weighed but 1 grain; so that if 1 grain contain 100 eggs, a scruple must contain 2000, and a drachm 6000, and an ounce at 8 drachms to the ounce, must contain 48000 eggs: Now if every egg hatch a worm, and every worm make a silk-ball, there must be produced from 1 ounce 48000 silk-balls; and if 3319 silk-balls make 1 pound and 1 ounce of fine silk (which he found they did) then 48000 silk-balls will make 15 pounds and 6 ounces of fine silk *averdupois* weight, or 18 pounds and 8 ounces *Troy* weight; which is very considerable: And in the same proportion, 1 pound of silk-worms eggs will produce worms sufficient to make upwards of 180 pounds of silk: But allowing for casualties, and supposing but 12 pounds of fine silk made from the worms; and their silk-balls produced from an ounce of silk-worms eggs, it will be found much to exceed most countries, according to *Augustino Gallo's* computation; for, he says, that in the southern parts of *France*, viz. *Languedoc* and *Provence*, they make but 7 or 8 pounds of silk from silk-worms hatched from 1 ounce of eggs; and in *Brescia* in *Italy*, but 8, 9 or 10 pounds of silk from an ounce; only that in *Calabria*, where the silk-worms and their eggs are larger, they make 11 or 12 pounds of silk from an ounce of eggs; which still does not exceed, nay hardly comes up to what we make in *England*.

Mr. *Barham* only adds, that he has learned by experience, how to hatch silk-worms twice a year, so as to have two good crops of silk every year; and it is certainly true, that mulberry-trees will produce leaves in *England* twice a year, without prejudice either to the tree or fruit.

An Answer to Dr. Keill's Defence of the Force of the Heart; by Dr. Jurin. Phil. Trans. N° 362. p. 1039. Translated from the Latin.

IN answer to Dr. *Keill's* complaint of being, together with *Borelli* and Dr. *Moreland*, unjustly accused of comparing the motion of the heart with an unactive quiescent weight, Dr. *Jurin* observes, that a certain quantity of motion is communicated to the blood and arteries by the heart; whence he concludes, that the quantity of the force of the heart could not possibly be known, till the quantity of its motion was discovered; and that motion can no more be compared with a quiescent weight, than a right line with a rectangle; by which he would be understood to mean, not that these learned men do expressly compare the motion of the heart with a quiescent weight; but that, when they expounded the force of the heart by a weight, they did not point out a method, how the quantity of motion arising from the force of the heart might be estimated: Dr. *Keill*, to remove this objection, observes, that the force of the heart consists in pression, equably exerted on the blood, in the manner, as the force of gravity impels a weight downwards, and by a continual impulse accelerates its motion; consequently since the force of the heart is equal to a weight by Sir *Isaac Newton's* Corol. it will, during the *systole* or contraction, communicate the same quantity of motion to the blood, as that weight by its force of gravity shall acquire by falling in the same time: In this manner, Dr. *Jurin* owns, the objection will be entirely removed; to wit, if the force of the heart be equal to the said weight, and consist in an equable pression continued thro' the whole *systole*: But Dr. *Keill* attempts not to prove the latter proposition, laying it down as an hypothesis; tho' Dr. *Jurin* has adduced some reasons to render the contrary opinion more probable, to wit, that the force of the heart does by no means act equably upon the blood, during the *systole*; but that in a small time collecting together all its force, it rushes at once upon the blood, and throws it out of the ventricles, in the manner explained at large, *Phil. Trans.* N° 358, 359, and as to the former proposition, it will anon appear to be false.

As to the meaning of the *Newtonian Corol.* Dr. *Jurin* only observes, that when Dr. *Keill* speaks of the force, with which water is thrown out of any orifice, Sir *Isaac Newton* uses no word in that *Corol.* to signify water thrown out by any force; but only determines a weight equal to the force, whereby the whole motion of effluent water may be generated, or that by the force of gravity it may in falling acquire a motion equal to that of water running out in the same time.

And if Dr. *Keill* have not mistaken the meaning of this corollary, he has not properly applied it, as will easily appear from considering the difference between the efflux of water from a hole in the bottom of a vessel always full, as Sir *Isaac Newton* considers it in that *Corol.* and the efflux of the blood out of the heart into the *aorta*; for, in the former case the water has already acquired all its velocity, and in a given time flows equably out of the hole; but the force of the heart, by Dr. *Keill's* hypothesis is applied to the quiescent blood in the ventricle, which in the first moment of time propels it with an infinitely small velocity towards the *aorta*; and continuing an equable pression, it at length communicates thereto a finite velocity, which it continually increases, till it has thrown all the blood out of the ventricle.

Again, Sir *Isaac Newton* in that *Corol.* does not consider the motion of the whole cataract of water, that moves and tends to run out with different degrees of velocity; but only that of the water actually in the hole, and now running out; but the force of the heart communicates motion to the whole mass of blood contained in the ventricle, and propels it towards the *aorta*.

In fine, Dr. *Jurin* denies, that a weight of five ounces can by the force of gravity acquire, in the time of a *systole*, that quantity of motion, that the heart communicates, even granting the hypothesis, that the force of the heart consists in an equable pression; for, by this hypothesis the motion, produced by the force of the left ventricle, will be (according to Dr. *Jurin's* calculation *Phil. Trans.* N° 359) equal to the motion of a weight of about 18 pounds, running over an inch in a second of time: But the motion, which a weight of five ounces acquires by the force of gravity in the time of a *systole* (if all resistance of the arteries and preceding blood be removed) or $\frac{1}{3}$ of a second of time, will be nearly equal to the motion of a weight of 12 pounds, that moves with the above velocity: And if any one would, on this hypothesis, determine the true weight, equal to the force of the heart, he will find by calculation a weight of 7 ounces and $\frac{1}{3}$; for this

weight will, in the time of a *systole*, nearly acquire the same quantity of motion in falling, as the force of the heart produces.

But, perhaps, some may object, that the difference above-mentioned, between the motion acquired by the weight as supposed by Dr. Keill, and the motion arising from the force of the heart, might have been owing to the inaccuracy of the positions, whereby Dr. Jurin brings the algebraical characters in his calculation to numbers: In order to obviate this objection, Dr. Jurin assumes a more simple case, in which a given bulk of water may by any force or equable pression be thrown out at a given orifice in a given time; which are the conditions supposed by Dr. Keill in order to determine the force of the heart.

In this case Dr. Jurin demonstrates, that neither the motion of the effluent water, nor the motion communicated by that force to the whole mass of water, is equal to the motion of the water in the *Newtonian Corol.* and that that force or pression is not equal to the weight determined by that *Corol.* and if this be accomplished, Dr. Keill's whole demonstration must fall to the ground.

He therefore, assumes a given cylinder of water, contained in a cylindrical tube of an infinite length; the orifice will be the section of the tube, which either superficies of the water touches, but to the other superficies a force will be applied by means of an *embolus*, of the same diameter with the tube: Now, let any given quantity of water run along the said section of the tube in any given time; and let another equal quantity run out at a hole of the same diameter in the bottom of the vessel, which according to Sir Isaac Newton is always to be kept full; and first let us see, whether the motion of the effluent water will be the same in both cases.

Let the right line A C (as in Fig. 1. Plate XIII.) represent the time of the efflux of the water; and the right line A B the equable velocity, with which the water runs out at the hole in the bottom of the vessel: Hence since the mass of water, running out at the hole, is in a compound ratio of the time and velocity, it will be represented by the rectangle A B C D; and its motion will be expressed by the solid parallelepiped, formed by the same rectangle drawn into the height A B, as being in a ratio compounded of the ratio's of the mass and velocity.

In the second case, *viz.* where the water runs thro' a cylindrical tube; the time will be represented, as before, by the same right line A C; but the velocity of the water will be in the ratio of the time; for, since the force applied acts equably, by the hypothesis;

pothesis, on the given mass of water; consequently, it will be represented by the mutable right line FG , proportional to the right line AF , or to the time from the beginning of the efflux; but the *molecula* of water running along the said section in the small space of time FH , will be represented by the rectangle of FH into the exponent of the velocity FG ; or if the small right line FH be supposed to vanish, by the *trapezium* $FGHI$; and the mass of water running along all the time AC by the right angled triangle ACE ; and because, by the hypothesis, that mass is equal to the mass of water running out in the former case, the triangle ACE will be equal to the rectangle $ABDC$; whence CE , or the velocity acquired at the end of the time AC , will be double the velocity CD or AB , with which the water ran out at the hole in the bottom of the vessel; but the motion of the water running along in the small time FH being in the compound ratio of the bulk and velocity, it will be represented by the evanescent prism, formed by the *trapezium* $FGIH$ drawn into the velocity FG ; whence the whole motion of the water running along all the time AC will be represented by a pyramid, whose base is the square of the right line CE , and height the perpendicular AC ; which pyramid, since it is to the parallelepiped determined in the former case, as 4 to 3, the motion of the effluent water will in both cases be in the same ratio; and consequently unequal, Q. E. D.

In the next place Dr. *Jurin* demonstrates, that the motion communicated at length to the whole quantity of water, contained in the tube, is not equal to the motion determined in the former case; but here, since that whole mass of water is by no means defined by the above-mentioned positions, he assumes it equal to the mass represented by the rectangle $ABCD$ that in the former case runs out at the hole, and in the latter flows along the said section: Whence since the whole quantity of motion communicated at length thereto, is in the ratio of the bulk and the velocity acquired at the end, it will be represented by the parallelepiped, formed by the rectangle $ABCD$ drawn into the right line CE ; but this is to the parallelepiped, determined in the former case, and formed by the same rectangle and right line CD , as the height CE to the height CD , or in a duplicate ratio: Moreover, since the mass of water contained in the tube might have been represented by any other rectangle, instead of the rectangle $ABCD$, it hence appears, that this motion might have had any other ratio to the motion determined in the former case; and therefore, is by no means equal thereto. Q. E. D.

It

It remains to shew, that the force, applied in this case, is not equal to the weight determined by Sir Isaac Newton's Corol. but this force and that of gravity acting on that weight, since both are equable, will be in the ratio of the motions produced by them in any given time; and since these motions are unequal, as has been just now demonstrated, these forces will be so too.
Q. E. D.

As to Dr. Keill's supposing the velocity of the blood flowing out of the heart to be equable, which Dr. Jurin had demonstrated to be remarkably unequal, this Dr. Keill denies, assuming only the mean velocity instead of the sum of all the different velocities: Moreover, he says that it does not sufficiently appear to him, whether the velocity of the ejected blood be equal or unequal, only that the former seem'd to him the truer opinion: But whether a person, who is to investigate the velocity of the blood, and applies the mass of ejected blood to the orifice of the *aorta*, does, without mentioning either the different velocities or the mean velocity, suppose the velocity of the blood to be equable, must be left to the unprejudiced; as also whether any force or pressure, applied to a fluid in a quiescent vessel, which is Dr. Keill's hypothesis, shall propel that fluid with the same degree of velocity at the beginning, as at the end.

Dr. Keill imagining he had answer'd Dr. Jurin's objections against his former method, he now comes to defend the other and more easy method: And here Dr. Jurin objected, that Dr. Keill assumes the force of the heart in different animals to be in the ratio of their weights; as also, the velocity of the blood flowing out of the iliac artery when cut, to be equal to that with which the blood is thrown out of the heart into the *aorta*; both which positions Dr. Jurin demonstrated to be false: Dr. Keill does not defend the latter, but maintains the former position on the authority of Borelli and other learned men, who have frequently assumed it, and this assumption Dr. Jurin animadverted on: Therefore Dr. Jurin's demonstration remains to be examined, which Dr. Keill takes to depend on a false principle, that affects all his theorems; for, he says, that Dr. Jurin supposes, the ventricles of the heart to impinge on the blood in the same manner, as a solid body moving with a given velocity, and by that impulse to communicate part of their motion to it; which hypothesis Dr. Keill thinks does not agree with either the motion of the blood, heart, or air expir'd out of the lungs.

As to the lungs, Dr. *Jurin* owns, that he consider'd them in the time of their contraction, as impinging on the air contain'd therein; and this he did on purpose: For, since *Bellini* and several other learned men, and Dr. *Keill* himself, have advanced several things on that force, with which the air in expiration acts upon the blood flowing thro' the lungs, and dissolves its *molecule*; which solution they suppose to happen in the very beginning of expiration; Dr. *Jurin* propos'd to examine this their opinion; but he saw, should he determine the air was expir'd by an equable force or pression, the motion communicated to the air by the lungs in initial expiration, or the reaction of the air on the lungs, and consequently, on the blood flowing thro' them, must have been suppos'd an infinitely small quantity; and consequently, could perform none of those effects, ascrib'd thereto: But if he had done so, he was aware that the followers of *Bellini* would justly complain, as hardly dealt by, in rejecting their hypothesis for a demonstration deduced from an arbitrary hypothesis and the most repugnant to theirs: Therefore, he chose rather to deduce the demonstration from an hypothesis, that favour'd them most, and that ascrib'd the greatest quantity of motion to the air in initial expiration; which was, that the lungs in initial expiration, impinge on the air with a given degree of velocity.

In defining the force of the heart, Dr. *Jurin* first proposes that hypothesis (as being the most simple and from which he deduces the solution) by which its ventricles, acquiring all their impetus in a moment of time, like a solid body with a given degree of velocity, impinge on the blood; and in the next place he considers not only that hypothesis, by which the ventricles of the heart are supposed to acquire all their motion in a very small space of time, and which seemed to him most probable; but likewise Dr. *Keill*'s hypothesis and many others, to all which he accommodates his solution: So that Dr. *Jurin* thinks whether that principle be found to be uncertain and false or the contrary, his solution will not thereby become the less certain.

Yet Dr. *Jurin* sees no reason, why his position may not be assumed, as well as the contrary position of Dr. *Keill* about force or pression: And tho' there is no intermediate space between the inner surface of the ventricles and the blood; yet it does not appear, why the matter may not be performed by impulse; for if an impulse be given a cube in contact with a globe, the cube will communicate a part of its motion to the globe

globe with the same ease, as if there were an intermediate space between them.

But, says Dr. *Keill*, these are solid bodies; and the case is quite different in the motion of fluids: This difference between the strokes of solid bodies, and the action either of a solid on a fluid, or of a fluid on a solid, Dr. *Keill* handles at large; and the not carefully attending to this difference, he affirms to be the source of all the mistakes in Dr. *Jurin's* positions: Dr. *Jurin* readily admits that difference to be true; and affirms, that he was well aware of the common doctrine, since nothing occurs more frequently in mechanic writers; but having proposed some new cases, to which the said doctrine could not be applied, he was obliged to take a different method from what had been hitherto us'd; and which he illustrates by the following easy example; suppose a cylinder of water of a given length to be at rest in a given tube, and let another solid cylinder of an equal diameter move thro' that tube, and impinge on the cylinder of water with a given velocity: The consequence will be, that the whole cylinder of water will be put into motion by that stroke, in the same manner, as if it were a solid cylinder; but the other cylinder will lose part of its motion in a moment of time, and both cylinders be carried with a common velocity thro' the tube: The case will be the same, if the cylinder of water running thro' the tube should impinge upon the solid cylinder at rest: But if the cylinder of water move with a given velocity thro' the tube, and a solid cylinder meet it with a different degree of velocity, so that the quantities of motion in the cylinder of water and the solid cylinder be respectively equal, the motion of both cylinders will be destroy'd in a moment, equally as if two solid bodies with equal degrees of motion should meet each other; other more complex cases, Dr. *Jurin* thinks, may be resolv'd from his *Treatise of running waters*; from which also may be shewn the possibility of stopping the blood, rushing with its whole impetus out of the ventricle, by the concurrence of a solid body, meeting it in a contrary direction with a given quantity of motion; which seems to be the great difficulty with Dr. *Keill*.

As to Dr. *Keill's* advice of applying the force of pressure as a principle; this Dr. *Jurin* affirms he has already perform'd in the said treatise: For when he supposes the motion of the heart to increase in the ratio of the time, it is the same thing, as if he applied the force of pression; and upon this supposition he has determined the motion arising from the force of

the

the heart to be twice greater, than when it is perform'd by the stroke or impulse of the ventricles: The other *Theorems* (and especially the fifth *Theor.* which Dr. Keill would reject, as deduced from the hypothesis of the stroke of the ventricles) do not depend upon that hypothesis, but may be demonstrated with equal facility from Dr. Keill's own hypothesis.

An Account of some Experiments to find how much the resistance of Air retards falling Bodies; by Dr. Desaguliers.
Phil. Transf. N^o 362. p. 1071.

DR Desaguliers took 12 balls (six of which were solid globes of lead of about two inches diameter; three hollow glass balls of about five inches diameter, and three light pasteboard hollow globes of about the same diameter) and carrying them to the upper gallery in the lanthorn on the Dome of St. Paul's Church, he let them fall down by two at a time, in the following manner.

1. A leaden ball and a glass ball. 2. A leaden ball and a glass ball. 3. A leaden ball and a glass ball.

Then he let fall in the same manner the three other leaden balls, each with a pasteboard ball.

After that, having the leaden and pasteboard balls brought up again, he repeated the experiment twice more with a leaden and pasteboard ball: Then he made the experiment twice more with a pasteboard ball alone, to see how long it would be in falling.

Upon the whole it appear'd, that the leaden balls were a very little longer than four seconds and $\frac{1}{4}$ in falling; the two largest of the glass balls six seconds, and the pasteboard balls six seconds and $\frac{1}{4}$.

The height of the gallery, from whence the bodies fell, was 272 foot above the pavement of the Church (then cover'd with boards) upon which they fell.

The times of the falls were taken two ways above. *viz.* with a wheel-chronometer, measuring a small part of time accurately and nearer than to $\frac{1}{4}$ of a second, which was made and contriv'd by the ingenious Mr. George Graham; and with a half second pendulum: The differences of time between the fall of the leaden balls and the other balls were taken below, by the President of the Royal Society, Mr. Folkes and another gentleman, who all agreed in their observations of the time, which they made each with an half second pendulum.

The following Table exhibits the Marks, Weights, and Diameters of the several Balls.

Leaden balls	Troy weight			Diameters in inches and decimals.	
	l	oz	dr		
1c	2	I	$\frac{1}{4}$	2,	I
2c	I	II	4	I,	19
3c	I	II	12	2	
4c	I	II	12	2	
5c	I	II	12	2	
6c	I	IO	00	I,	98
Pasteboard balls					
A	0	3	6	5,	5
B	0	I	14	5,	I
C	0	I	17	5,	I
Glass balls					
D	0	3	$13\frac{1}{4}$	3,	9
E	0	5	$3\frac{1}{4}$	5,	42
F	0	6	$0\frac{1}{4}$	5,	55

N. B. The polar and equatorial diameters of the glass balls being different, the mean diameter for each of them is set down; the true diameters are as follows, of D 4 and 3, 8. of E 5, 6 and 5, 25. of F 5, 7 and 5, 4 inches.

The particular experiments are as follows.

Exp. 1. The fall of 1c and D.

c fell by the pendulum in 4" and $\frac{1}{4}$.

The fall of D was so near it, that the difference was not taken either above or below.

Exp. 2. The fall of 2c and E.

2c fell by the chronometer in 5"; by the pendulum in 4" and $\frac{1}{4}$.

The time of the fall of E was not taken above.

The difference taken below was 1" $\frac{1}{4}$.

Exp. 3. The fall of 3c and F.

3c fell by the chronometer in 4" and $\frac{1}{4}$; by the pendulum in 4" $\frac{1}{4}$.

F fell in 6" and $\frac{1}{4}$.

The difference taken below was = 1" and $\frac{1}{4}$.

Exp. 4. The fall of 4c and A.

5c fell by the chronometer in $4''$ and $\frac{1}{4}$; by the pendulum in $4''$ and $\frac{1}{4}$.

A fell in $6''$ and $\frac{1}{4}$.

The difference taken below was $= 2''$.

Exp. 5. The fall of *5c* and *B*.

No observation was made either above or below.

Exp. 6. The fall of *6c* and *C*.

6c fell by the chronometer in $4''$ and $\frac{1}{4}$; by the pendulum in $4''$ and $\frac{1}{4}$.

C was not taken above.

The difference taken below was $2'' \frac{1}{4}$.

Exp. 7. The fall of *1c* and *B*.

1c fell by the chronometer in $4''$ and $\frac{1}{4}$; by the pendulum in $4''$ and $\frac{1}{4}$.

B was not taken above.

The difference taken below was $2''$ and $\frac{1}{4}$.

Exp. 8. The fall of *5c* and *A*.

5c fell by the pendulum in $4''$ and $\frac{1}{4}$.

A fell foul, and so was not observ'd at all.

The difference taken below was $2''$.

Exp. 9. The fall of *B* alone.

It fell by the chronometer in $6''$ and $\frac{1}{4}$; by the pendulum in $6''$ and $\frac{1}{4}$.

Exp. 10. The fall of *C* alone.

It fell by the chronometer in $6''$ and $\frac{1}{4}$; by the pendulum in $6''$ and $\frac{1}{4}$.

By *Galileo's* theory, the lead, which was $4''$ and $\frac{1}{4}$ in falling, must fall four foot the first half second, or 16 foot the first second, which amounts to 324 foot in $4''$ and $\frac{1}{4}$: But as the sound of the ball (as it struck the bottom) by which we reckoned our time, had 272 foot to move, we must abate a quarter of a second nearly (supposing sound to move one mile in $4''$ and $\frac{1}{4}$) which will take away 35 foot, that the body must have fallen in the last quarter of a second, and reduce the number of feet to 289; so that the lead will have only fallen 11 feet short of the theory, which must be attributed to the resistance of the air.

The large glass ball in the $6''$ of its fall, would in a *vacuum* go thro' 576 feet; but deducting the last quarter of a second, or 47 feet for the motion of sound, it must only fall 529 feet in a *vacuum*: Now since it fell but 272, there have been 257 feet taken off from the fall by the resistance of the air.

Likewise the pasteboard ball must have fallen 875 foot in 6" and $\frac{1}{4}$; but deducting the last quarter of a second or 51 foot for the motion of the sound, there remains only 625 feet for its fall in *vacuo*; but as it fell only 272 feet, we must allow an abatement of 353 feet for the resistance of the air.

At a medium we may call the weight of the glass ball 5 oz. Troy, and its diameter five inches and a half; and the weight of the pasteboard ball two ounces Troy, and a little more than five inches in diameter.

All the lead balls fell within near a foot of each other, and made an impression in the boards of about $\frac{1}{3}$ of their depth.

The barometer stood at 30,1 inches; and the mercury was very convex; and therefore inclin'd to rise still.

A further Account of Experiments for finding how much the Resistance of the Air retards falling Bodies; by the same. Phil. Transf. N^o 362. p. 1075.

DR. Desaguliers, finding by the former experiments, that thin glass balls, and even balls of pasteboard, were much too heavy to make so considerable a difference between the time of their fall and that of leaden balls, as to be easily observ'd, contriv'd a way to make dried hogs bladders perfectly round, by blowing them when moist, within a strong spherical box of *Lignum vite*, and letting them dry in the said box before he took them out; which he did by opening the box that screw'd in the middle, and had a hole in the pole of one of its hemispheres to let the bladder pass thro', in order to tie it after blowing; and some few small holes all over the box, that in blowing no air might be confined between the inside of the box and the bladder, so as to hinder it from assuming a spherical figure; besides, he took off the ends of the ureters; the fat and a great deal of the upper coats of the bladders, before he blow'd them in the box, to render them still lighter.

The bladders the Dr. made use of, were some of the thinnest he could find ready blown, which he moistened in water, taking care to leave none in the inside.

Having prepared five bladders in the manner aforesaid, he took them up to the upper gallery on the top of the *Cupola* in St. Paul's Church; and there he let them fall one at a time, together with a leaden ball of about two inches in diameter, and weighing two pounds Troy: and he took notice of the time

time of the fall of each bladder, having found by former experiments, that the balls are about $\frac{1}{4}$ " and $\frac{1}{2}$ ", or a little longer in falling the same height, viz. 272 feet,

The following table exhibits in the first column the marks of the bladders; in the next their diameters; in the third their weights in gr. Troy; in the fourth the times of their fall in seconds of time; and in the fifth the difference of time between the fall of the leaden balls and of each bladder, which was taken below by the President of the Royal Society, Dr. Halley, Dr. Jurin, Mr. Folkes, and Mr. George Graham: The time was taken above with Mr. Graham's chronometer; and below with the same instrument; and with three half second pendulums, all which agreed very well together.

The experiments having been twice repeated, the table is twice set down; and those experiments in which the bladders fell streight down and the most regularly, have an * before them.

Marks	Diameters in inches.	Weight in gr. Troy.	Time of the whole fall.	Difference between the lead balls and bla.
A	5, 3	128	19 $\frac{1}{2}$	14 $\frac{1}{2}$
*B	5, 193	156	17 $\frac{1}{2}$	12 $\frac{1}{2}$
C	5, 33	137 $\frac{1}{2}$	18 $\frac{1}{2}$	14 $\frac{1}{2}$
D	5, 26	97 $\frac{1}{2}$	22 $\frac{1}{2}$	17 $\frac{1}{2}$
*E	5, 02	99 $\frac{1}{2}$	21 $\frac{1}{2}$	17 $\frac{1}{2}$
*A			19 $\frac{1}{2}$	14 $\frac{1}{2}$
B			18 $\frac{1}{2}$	14 $\frac{1}{2}$
*C			18 $\frac{1}{2}$	14 $\frac{1}{2}$
D			24 $\frac{1}{2}$	19 $\frac{1}{2}$
E			21 $\frac{1}{2}$	16 $\frac{1}{2}$

The diameters and weights may be relied on, being taken both the day the experiments were made and the day after.

As the necks of the bladders in drying shrink, so as to open a little, they must be blown before each experiment: And as to the manner of letting them fall exactly in the same instant of time it is describ'd by Fig. 2. Plate XIII, in which A A A A is the hole thro' which the bodies fell; 1, 2 is a board laid over the hole; G D D is another board, fixed to the first board by the two wooden screws D D, with a pulley G at the other end of it over the

the hole; W is a two pound ball of lead, fasten'd to a strong thread, which going over the pulley is stretch'd horizontally from G to the nails F, to which it is fasten'd, so as to be about $\frac{1}{4}$ of an inch above the board; B is one of the bladders, hanging with the neck or heaviest part downwards, by means of a loop of fine thread, as E H, which goes over the horizontal thread G E F. Now when the thread of the lead (which in all is but a foot long) is cut with a pair of scissars just at E before the loop of the bladder, the lead pulling away the string, the loop of the bladder slips off the remaining thread F E, and begins to fall exactly in the same instant as the lead: But if the thread should be cut between E and F, as the lead falls, its thread might give the bladder an oblique direction.

He that observes the time either with a pendulum or chronometer, may take it very exactly, by seeing the motion of the scissars, as they cut the thread.

N. B. As the diameters of the bladders were taken by wrapping a thread twice round them, and something must be allowed for the thickness of the thread; the diameters of the bladders will then be viz. A 5, 28 inches; B 5, 19; C 5, 31; D 5 $\frac{1}{4}$ and E just five inches in diameter.

The bladder E was rough, with several wrinkles and inequalities, which made it to be longer in falling than it should be, according to its diameter and weight.

A pail of water thrown down met with such resistance in falling 272 foot thro' the air, that it was all turned into drops like rain.

The Invention of making Clocks to keep Time with the Sun's apparent Motion; by Mr. Joseph Williamson. Phil. Trans. N° 363. p. 1080.

THE author of a book, published in French, supposes, that the making clocks to agree with the sun's apparent motion was a thing never thought of by any before himself: But in the first place Mr. *Williamson* takes notice of the copy of a letter in this book, wrote by one P. *Kresa* a Jesuit to one Mr. *Williamson* clockmaker to his Imperial Majesty; wherein he mentions a clock found in the cabinet of King *Charles II.* of Spain, about the year 1699 or 1700, which shewed both equal and apparent time, according to the tables of equation, and went 400 days without winding up.

This Mr. *Williamson* is well satisfied is a clock of his own making; for, about six years before that time he made a clock for

for one Mr. *Daniel Quare*, which agrees with the description given of it in that letter, and went 400 days: Mr. *Quare* sold the said clock and it was sent to King *Charles the II. of Spain*; it was made in such a manner, that if the pendulum was adjusted to the sun's mean motion, the hands would shew equal time on two fixed circles, the one the hour and the other the minute; there were also other two moveable circles of the same kind, that moved forwards and backwards as the time of the year required; on which the same hands likewise shewed apparent time, according to the equation tables.

This method the author of that book affirms he knew of, and applied the same motion to pocket-watches 12 or 14 years before, which Mr. *Williamson* owns he never did; being well satisfied, that watches with springs and balances are very unfit to shew the minute difference, as it increases and decreases, between equal and apparent time.

Soon after this clock was sent to *Spain*, Mr. *Williamson* made others for Mr. *Quare*, which shewed apparent time by lengthening and shortening the pendulum, in lifting it up and letting it down again, by a roller somewhat in the form of an ellipsis, thro' a slit in a piece of brass, which the spring at the top of the pendulum went thro': By this means every vibration of the pendulum would agree to a second of time of the sun's apparent motion; that roller which lifted up the pendulum, and let it down again, continually moving about all the year: So that it may seem very odd, that this author never heard of it, so many years after they were made; for, Mr. *Williamson* made one of those (and that not the first) with the rising and setting of the sun, which was sold to King *William III. of England*, and set up at *Hampton-court*, where it has continued hitherto. Mr. *Williamson* thought of this contrivance of lengthening and shortening the pendulum several years before he made any of them; and afterwards he made others for Mr. *Quare*, which shewed the difference between equal and apparent time according to the equation tables, by an index moving both ways from the top of a circle; shewing on one side, how much a clock, that keeps equal time, should be faster than the sun, and on the other side how much slower.

But these clocks, that Mr. *Williamson* then made to agree with the sun's apparent time, were done according to the equation tables, which he found did not agree very exactly with the sun's apparent motion; nor can any other be made to keep equal time, that will gain and lose all the year agreeably to the said tables; for,

for, tho' the tables themselves may be true, yet some difference in motion in both sorts of clocks proceeds either from cold or heat, altering the length of their pendulums: By the observations Mr. *Williamson* made, he supposes this difference to be about the $\frac{1}{100}$ of an inch in the length of a pendulum vibrating seconds, which will alter the motion of the clock about 12 seconds in 24 hours: But to cause his clocks of apparent time to go as exact as possible, he made a table by observation in the following manner; for several years together observing when the sun, as often as he was to be seen, came to the meridian, and always setting down the difference between his coming to the meridian, and the time by a clock, he had adjusted as much as possible to equal time, and always taking notice how much his equal time clock gained or lost at the end of every year, he compleated his table in 1711: Since which time he made a great many of these clocks, which gave satisfaction to all that bought them; and he had not heard of any such clock sold in *England*, but what was of his own making: So that Mr. *Williamson* thinks he may justly claim the greatest right to this invention of making clocks go with apparent time.

An Account of some New Experiments relating to the action of Glass Tubes upon Water and Quicksilver; by Dr. Jurin.
Phil. Trans. N^o 363. p. 1083.

IN *Phil. Trans.* N^o 355 Dr. *Jurin* asserted, that the suspension of water in a capillary tube was owing to the attraction of a small annular surface on the inside of the tube, which touch'd the upper part of the water: Among the several experiments made use of to prove this assertion, was that of a glass funnel of several inches diameter, with its small end drawn out into a very fine tube; which funnel being inverted and filled with water, the whole quantity of water therein contained was sustained above the level by the attraction of that narrow small ring of glass, with which the upper surface of the water was in contact.

Soon after that discourse was published, there came out a book, by a member of the *Royal Society* in which he accounted for that experiment in the following manner.

If there be a funnel (as represented by ABC in Fig. 3. Plate XIII.) full of water, and whose wide end stands in a vessel of water as BC, and if the top of the funnel A end in a capillary tube open at A, the whole water will be sustained, viz. the column Aa by the attraction of the circle of glass within the tube immediately above it, and all the rest of the columns of

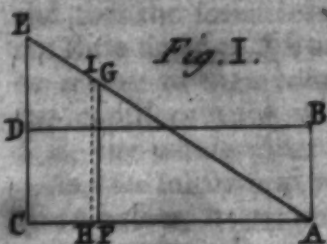


Fig. I.

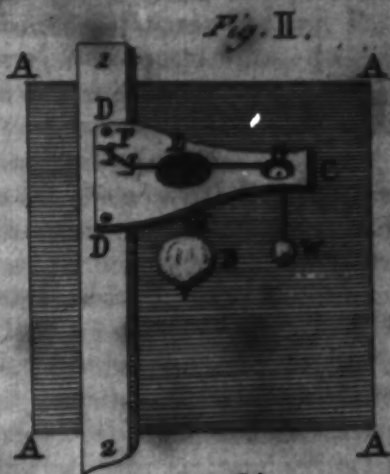


Fig. II.



Fig. III.

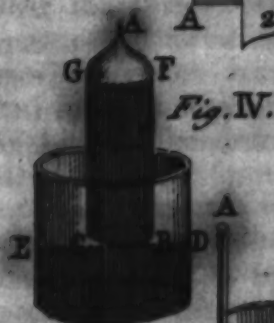


Fig. IV.



Fig. V.



Fig. VI.

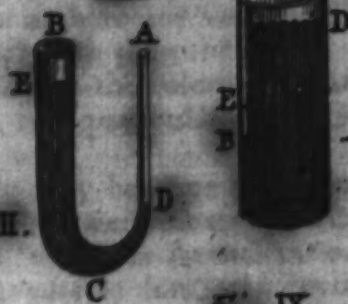


Fig. VII.

Fig. VIII.



Fig. IX.

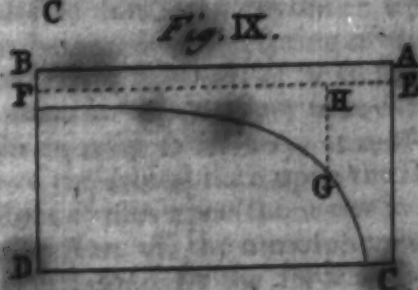


Fig. X.



Fig. XI.

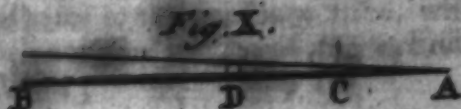
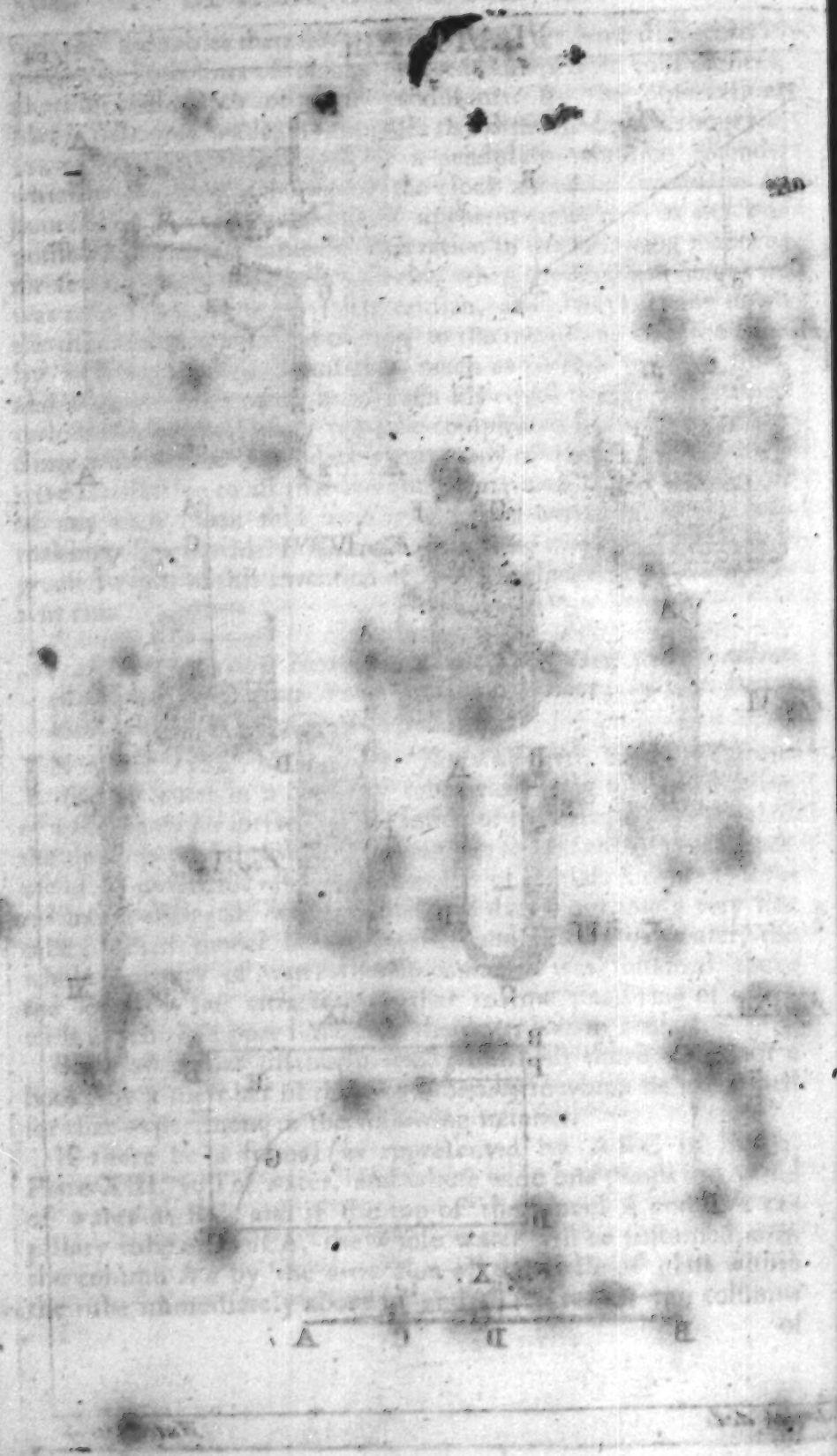


Fig. XII.



of water, as *Ff*, *Dd*, *Ee*, *Gg*, &c. in some measure by the attraction of the parts of the glass above them, as *F*, *D*, *E*, *G*; and that the small columns or threads of water *Dd* and *Ee* do not slide down to *Ff* and *Gg*, and so go quite down, seems to be owing to their cohesion with the column *Aa*, sustained by the capillary tube *A*; for if you break off the said tube at *DE*, the whole water will immediately sink down.

As this solution is very different from what *Dr. Jurin* had given before, he examines into this Gentleman's account of the experiment, either to demonstrate its insufficiency or to retract his own solution; and he gives the following experiment.

The funnel *APBGC* (as in *Fig. 4.*) whose lower part *BCFG* was cylindrical to a considerable height, and whose top was drawn out into a fine tube at *A*, being filled with water to the height *BF*, so that the surface of the water *FG* did not reach to the arched part of the funnel, the *Dr.* touched the end *A* with a wetted finger, by which means a small quantity of water being insinuated into the capillary tube at *A*, the water contained in the funnel was suspended above the level of the water in the cistern *DE*, as in the former experiment.

In this experiment it is manifest, that the small columns, into which we may suppose the cylinder of water *FBGC* to be divided, are no ways sustained by the attraction of the arched part of the glass above them, since they have no contact therewith; nor is there any such middle column of water, which by its contact with the tube at top, is both sustained itself, and helps to sustain the columns about it: Upon the supposition of which two particulars that Gentleman's solution was founded.

This experiment may be thus accounted for; the cylinder of water *FBGC* by its weight balances a part of the pressure of the atmosphere, which is incumbent on the water in the cistern, and endeavours to force that cylinder upwards; the rest of that pressure is balanced by the spring of the air *AFG*, which is included between the cylinder of water *FGBC*, and the small column of water in the capillary tube *A*: But as this air by its spring presses equally every way, it must balance as much of the pressure of the atmosphere upon the little column of water at *A*, as it does of that upon the water in the cistern; the remainder of the pressure of the atmosphere upon the column of water at *A* is sustained by the force with which that column adheres to the capillary tube, which, therefore, does exactly balance the weight of the cylinder of water *FGBC*,

and is the real, tho' not the immediate cause of its suspension.

The experiment succeeds in the same manner when a column of mercury, instead of the column of water FG BC, is raised into the funnel, the top of the tube being touched with a wet finger, as before: But then the height of the mercury in the funnel must be as much less than that of the water, as its specific gravity is greater.

Dr. Jurin comes next to examine, whether the experiments in *Phil. Transf.* N^o 355. would succeed *in vacuo*; and whether water could be suspended in a wide tube by means of a capillary at top at a greater height than what it can be raised to by the pressure of the atmosphere.

In order to this he boiled some water, and afterwards exhausting it of its air, those experiments all succeeded in the exhausted receiver, in the same manner as in the open air: The 13th *Exp.* in particular was made with a tube of about 35 inches in length, and $\frac{1}{2}$ of an inch in diameter, its top being drawn out into a fine capillary; which being filled with water, exhausted of its air, as above-mentioned, the whole quantity continued suspended in the exhausted receiver.

This plainly shews, that the success of that experiment does not depend on the pressure of the air; since the small quantity of air left in the receiver was by no means capable of sustaining the water at so great a height; and consequently, that the height at which water may be suspended in this manner, is not limited by that pressure.

But here a considerable difficulty presents itself to such as attentively consider this experiment: In order to make which the better appear; it will be proper to observe what happens, when a simple capillary tube is filled with water exhausted of air, and included in the exhausted receiver.

In this case the whole column of water contained in the tube ACB Fig. 3. is suspended by the attraction of the *annulus* at the top of the tube A; and tho' that *annulus* do not immediately act upon any part of the water, but what is either contiguous to it, or so near as to be within the sphere of its attraction, which extends but to a very small distance; yet it is impossible, that any other part of the water, as for instance that at C, should part from the water above it and sink down, because its descent is opposed by the attraction of the contiguous *annulus* at C; for, this being equal to the upper *annulus* at A, is capable of sustaining a column of water of the length

AB;

AB; and consequently, is more than sufficient for supporting the column of water below it CB: From which it is plain, that no part of the water contained in the tube can possibly descend, unless the upper part, assisted by the weight of the water below it, be sufficient to overcome the attraction of the *annulus* of glass at A.

But in such a compound tube (as that made use of in the Dr's experiment) A CB Fig. 6. the case is very different; and it does not easily appear, why in a *vacuum* any part of the water in the wider part of the tube, as for example at C, should not leave that which is above it and descend; since the *annulus* at C is by much too wide to sustain a column of water of so great a length as CB.

The best answer the Dr. can give to this difficulty is, that the cohesion between the water contained in the capillary and that below it, is sufficient to balance the weight of the suspended column: But how far this cohesion may depend upon the pressure of a medium subtil enough to penetrate the receiver, is worthy of consideration: For, tho' such a medium will pervade the pores of the water, as well as those of the glass; yet it will act with its entire pressure upon all the solid particles, if they may be so called, of the surface of the water in the cistern; whereas, so many of the solid particles of the water in the tube, which happen to lie directly under the solid particles of the water above them, will thereby be secured from this pressure; and consequently, there will be a less pressure of this medium upon any surface of the water in the tube below the capillary, than upon an equal surface of the water in the cistern: So that the column of water suspended in the tube may be sustained by the difference between those two pressures. This explication seems to be favoured by the following experiments, which may all be accounted for in the same manner.

The first is the famous experiment of the suspension of mercury, exhausted of its air to the height of 70 or 75 inches, in the *Torricellian* tube in the open air; to which may be added the suspension of mercury likewise exhausted of air within the exhausted receiver, as related by M. *Papin*, in his *continuation du digesteur*: Next are the experiments made by the famous M. *Huygens*, and described by him in *Phil. Trans.* N° 86. of the cohering of polished plates with a considerable force in the exhausted receiver; as likewise of the running of water and mercury, when purged of air, thro' a siphon of unequal legs

in vacuo: All which he accounts for from the same principle, and much in the same manner, as Dr. *Jurin* has made use of for explaining the above experiment.

As to the existence of such a medium, vide Sir *Isaac Newton's* queries at the latter end of the last edition of his optics.

Dr. *Jurin* having made some experiments on mercury before the *Royal Society*, which were exactly the reverse of those made by Dr. *Taylor*, Mr. *Hauksbee* and himself on water, he comprizes the whole of this affair in the following propositions, the proof of which is contained in the experiments annexed.

Proposition 1. The particles of water attract each other.

This being now universally acknowledged, needs no demonstration, the sphericity of the drops of rain, and the running of two drops of water into each other upon their contact, manifestly proving it.

Prop. 2. The particles of quicksilver attract each other.

This is likewise manifest from the spherical figure, into which a drop of mercury forms itself upon a table; and from two of them immediately running together, as soon as they come to touch.

Prop. 3. Water is attracted by glass.

This plainly appears from all the experiments on this subject.

Prop. 4. Quicksilver is attracted by glass.

Experiment 1. If a small globule of quicksilver be laid upon a clean paper, and be touched with a piece of clean glass; upon drawing the glass gently away, the quicksilver will adhere to it, and be drawn away with it; and if the glass be lifted up from the paper, the quicksilver will be taken up by it, in the same manner as a piece of iron is drawn up by the loadstone, and will adhere to the glass by a plain surface of a considerable breadth in proportion to the bulk of the drop, as plainly appears by an ordinary microscope. Then if the glass be held somewhat obliquely, the drop of mercury will roll slowly upon its axis along the underside of the glass, till it come to the extremity, where it will be suspended as before.

Exp. 2. If a pretty large drop of mercury be laid upon a paper, and two pieces of glass made to touch it, one on each side; upon drawing the glasses gently from each other, the drop of mercury will adhere to them both, and be visibly drawn out from a globular to an oval figure, the longer axis passing thro' the middle of those surfaces, in which the drop touches the glasses.

Prop.

Prop. 5. The particles of water are more strongly attracted by glass, than by each other.

This plainly appears from the rising of water in small tubes above the level: For, when the water begins to rise into a capillary tube, all the particles of water that touch the small *ring* at the bottom of the tube, must have quitted the contact of the other water, and have risen contrary to their gravity to come into contact with the glass. After the same manner the other experiments of Dr. *Taylor*, Mr. *Hanksbee* and Dr. *Jurin* on this subject, are easily explicable: For, upon a careful examination it will be found in them all, that some parts of the water quit the contact of the other water, and join themselves to the glass.

Prop. 6. The particles of quicksilver are more strongly attracted by each other than by glass.

Exp. 1. If a small tube as AB Fig. 7. open at both ends, be dipped into a glass-vessel, filled with mercury, and be held close to the side of the vessel, that the rise of the mercury within it may appear; the mercury will partly enter into the tube, but will stand within it at some depth, as CE, below the surface of the mercury in the vessel CD; and this depth will always be reciprocally as the diameter of the tube.

In this experiment a column of mercury of the height CE endeavours to force the mercury higher into the tube; and as glass has been already proved to attract quicksilver, the attraction of the annular surface on the inside of the tube, which is contiguous to the upper part of the mercury, will likewise conspire to forward its ascent: What opposes the ascent of the quicksilver is the power, by which that part of it, which endeavours to rise into the glass, is drawn back by the attraction of the other mercury, with which it is in contact laterally; and this does not only balance the attraction of the glass, but likewise the weight of the column of mercury of the height CE; and consequently, this attraction is considerably stronger than the attraction of the glass.

The cause, therefore, that suspends the weight of the column of mercury CE (being the difference between the attraction of the annular surface of the tube at E, and that of an equal surface of the quicksilver in the cistern, from which the mercury that endeavours to rise into the tube, must recede in order to unite itself to such an *annulus* of the glass) will always be proportional to that annular surface, or to the diameter of the tube: And since the column sustained must be proportional to the cause
that

that suspends it, that column must likewise be as the diameter of the tube; but the column suspended is as the square of the diameter of the tube and the height CE conjointly: From whence it follows, that the height must be as the diameter of the tube reciprocally, as it is found to be by experiment.

The experiment of the ascent of water above the level in a capillary tube is just the reverse of this.

Exp. 2. Quicksilver being poured into the inverted siphon ACB (as in Fig. 8.) one of whose legs AC is narrower than the other CB ; the height CE at which the mercury stands in the wider leg CB , is greater than the height CD , at which it stands in the narrower leg CA .

On the contrary, water stands higher in the narrower leg than in the wider.

Exp. 3. $ABCD$ (as in Fig. 9.) represents a rectangular plane of glass, which makes one side of a wooden box; on the inside of this is another glass plane of the same size, which at the extremity AC is pressed close to the former, and opens to a small angle at the opposite extremity BD . When mercury is poured into this box to any height as CE , it insinuates itself between the two glass planes, and rising to different heights between the glasses, where the opening is greater or less, it forms the common hyperbola CGF , one of whose asymptotes is the line on which the surface of the mercury in the box touches the inner glass; the other is the line AC , in which the planes are joined: This hyperbola having been carefully examined by Mr. *Hauksbee* and the Dr. the rectangle EHG , wheresoever taken, proved always equal to itself, to as great accuracy as could be expected, when the planes were opened to any considerable angle: But when the opening was very small, the inequalities of the planes, tho' the best the Dr. could procure, bearing a greater proportion than before to the distance between them, occasioned a very sensible variation. Which, by the bye, the Dr. takes to be the reason, why the ordinates found by Mr. *Hauksbee*, in examining the curve produced in a contrary position, upon dipping two glass planes, so joined, into spirits of wine, do not answer to those of the hyperbola.

Exp. 4. AB (as in Fig. 10) is a perpendicular section thro' two glass planes join'd at A , and opened to a small angle at B ; C represents a pretty large drop of mercury (the larger the better) which being made to descend as far as C , by holding the planes in an erect position, with the extremity A downwards, retires from the contact of the planes to D , upon inclining the planes towards an horizontal position; and the distance

CD

CD becomes greater or less, as the planes are more or less inclin'd towards the horizon.

A drop of any oily or watery liquor moves the contrary way, as has been shewn by Mr. Hawksbee.

Exp. 5. AB (as in Fig. 11.) is a tube open at both ends, and a foot or two in length, whose lower part is drawn out into a fine capillary at B: This tube being fill'd with mercury, the whole column of quicksilver will be sustained therein, provided the capillary tube at B be sufficiently small; but if the mercury in the extremity B be suffer'd to touch any other mercury, it runs all out of the tube: If without letting it touch any other mercury, a small part of the end B be broken off, the mercury will run out till it come to some lesser height as BC, at which it will again stop; the height BC, being nearly in a reciprocal proportion to the diameter of the small end of the tube.

The seventh experiment in *Phil. Trans.* N° 355 is the reverse of this.

Exp. 6. Is the same in substance with the former, only that it was made with a large glass funnel AB (as in Fig. 12) instead of a tube.

The reverse of this experiment in water is the 13th in the said *Phil. Trans.*

In all these experiments it is manifest, that the effect is owing to the difference between the two attractions, by which the mercury tends to glass and to its own body; they being always oppos'd to each other: So that a particular explication is no ways necessary; only the Dr. answers the following objection.

In the experiments brought to demonstrate the fourth proposition, the globule of mercury adheres to the glass in a plane surface, which cannot be done without increasing the surface of the globule; and consequently, removing some of its particles from the contact of each other: If therefore they tend more strongly to each other than to the glass, why do they not recede from the glass, and assume a figure perfectly spherical, that they may all have the greatest possible contact with each other?

To this it may be answer'd, that the power, by which mercury is attracted, either by glass or by other mercury, is proportional to the attracting surface; and therefore, tho' *ceteris paribus*, the tendency of mercury to glass be not so strong as its tendency to other mercury; yet in this case a greater number

ber of mercurial particles coming into contact with the glass than what recede from the contact of each other; it is not surprising, that the attraction of the glass prevails, and causes the globule to adhere to it: For, the number of mercurial particles which lose their contact with the other mercury, is no more than what makes up the difference of surface, which arises from changing the figure of the drop: Whereas the particles, which by this means come to adhere to the glass, are all those that constitute the plane surface, in which the globule touches it.

Which consideration should likewise be applied to the suspension of quick-silver in glass tubes, either at extraordinary heights in the open air, or at lesser heights in a *vacuum*, as above-mentioned: For, the top of the tube being spherical, or nearly so, it will be found, that the contact of the mercury with the extremity of the tube, is to the contact with other mercury, which would be gain'd by its leaving the top of the tube and descending a very small space, in a ratio infinitely great; and consequently, that the contact of the mercury with the top of the tube is one cause of the suspension.

Corol. 1. From this proposition it appears, that in a barometer made with a narrow tube, the quicksilver will never stand at so great a height, as in a wider: Which accounts for the phenomenon so often mentioned by M. *De la Hire* in the Memoirs of the Royal Academy of Sciences at *Paris*; to wit, that in the barometer he constantly made use of for his annual observations, the quicksilver did not rise so high, as in another he kept by him, by about three lines and a half, which is near $\frac{1}{2}$ of an inch *English* measure: For, he tells us, that the tube of his barometer was very small; so that there is no need of having recourse to any peculiarity, either in the quicksilver or the glass of which that tube was made; or to the imperceptible remnant of air left in the tube; from some of which causes that effect and some others of the same kind were suppos'd to proceed.

Corol. 2. In a barometer made with a small tube, the mercury will rise and fall irregularly; For, as the height of the mercury depends partly upon the diameter of that part of the tube that touches the upper surface of the mercury; it is plain, that the unavoidable inequalities in the diameter of the tube will be more considerable, in respect to the whole diameter; and consequently, will affect the height of the mercury more in a small tube than in a wider: And this Dr. *Jurin* takes

to be the reason, why it is so very difficult to make two barometers, which shall exactly agree in the height of the mercury in all dispositions of the air; especially, if the tubes be very narrow. This irregularity is still more considerable in the pendent barometer, in which the mercury moves thro' a large space, in order to make a small alteration in the length of the column suspended. The same consideration is easily extended to those levels, that depend upon the rising of mercury to the same height in the opposite legs of a bent tube; an instrument of which kind was offer'd for the service of the public: And as the effect is just contrary in levels made with water or spirits of wine, due regard ought to be had to this property in the construction of those instruments, by making the tubes sufficiently wide, in order to diminish the air as much as possible.

An Account of a surprising Fall of Water from a Spout on the Mores in Lancashire; by Dr. Richardson. Phil. Trans. N^o 363. p. 1097.

DR. Richardson gives the following account of a remarkable spout of water, that fell on *Emott-More* near *Coln* in *Lancashire* on the third of *June* 1711. about ten o'clock in the morning; when several persons who were employ'd in cutting peat near the place where this accident happened, upon a sudden were so terrified with an unusual noise in the air, that they left their work and ran home, which was about a mile from the place; but to their great surprize they were intercepted by water; for, a small brook in the way was risen upwards of six feet perpendicular in a few minutes of time, and had overflown the bridge.

It is to be observ'd, that there was no rain at that time on *Emott-more*, only a mist, which is very frequent upon those high mountains in summer time: There was a great darkness in the place where the water fell, without either thunder or lightning, as the Dr. was inform'd by an eye witness. The meadows at *Wicollae* were overflown so much, that the like had not been observ'd in several years before; tho' there it was a very bright day.

The Dr. went to view the place, and found, that the ground was torn up to the very rock where the water fell, which was upwards of seven foot deep, and a deep gulph was form'd for above half a mile, and vast heaps of earth cast up on each side of it; some pieces still remaining upwards of 20 foot

over, and six or seven foot thick? About 10 acres of ground were destroyed by this flood? The first breach where the water fell was about 60 foot over, and no appearance of any eruption, the ground being firm about it, and no cavity appearing. It is to be observ'd, that the ground on each side the gulph was shaken in such a manner, that large chasms appeared at upwards of 30 foot distance,

N^o 263. p. 1007.
the Mores in Lancashire; by Dr. Richardson. Phil. Trans.
An Account of a surprising Fall of Water from a Spring in

D R. Richardson gives the following account of a remark-
able spout of water, that fell on Mount-Joseph near
Columbia in Tennessee on the third of June 1711. About ten
o'clock in the morning; when several persons who were em-
ploy'd in cutting peat near the place where this accident hap-
pened, upon a sudden perceived an unusual noise
in the air, that they immediately fled, great surpris they
were intercepted by a prodigious quantity of water which was
then upwards of six feet deep, and continued about fifteen
minutes or more, and had overflowed the mountains above them.
It is to be observ'd, that there was no rain at that time on
Mount-Joseph, only a mist, which is very frequent upon those
high mountains in summer time: There was a great darkness
in the place where the water fell, without either thunder or
lightning, as the Dr. was inform'd by an eye witness. The
meadows at Wicklow were overflowed so much, that the lile
had not been observ'd in several years before; tho' there is
was a very bright day.

The Dr. went to view the place and found that the ground
 was torn up to the very rock where the water fell, which was
 upwards of seven foot deep, and a deep gulph was formed in
 above half a mile, and vast heaps of earth cast up on each
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